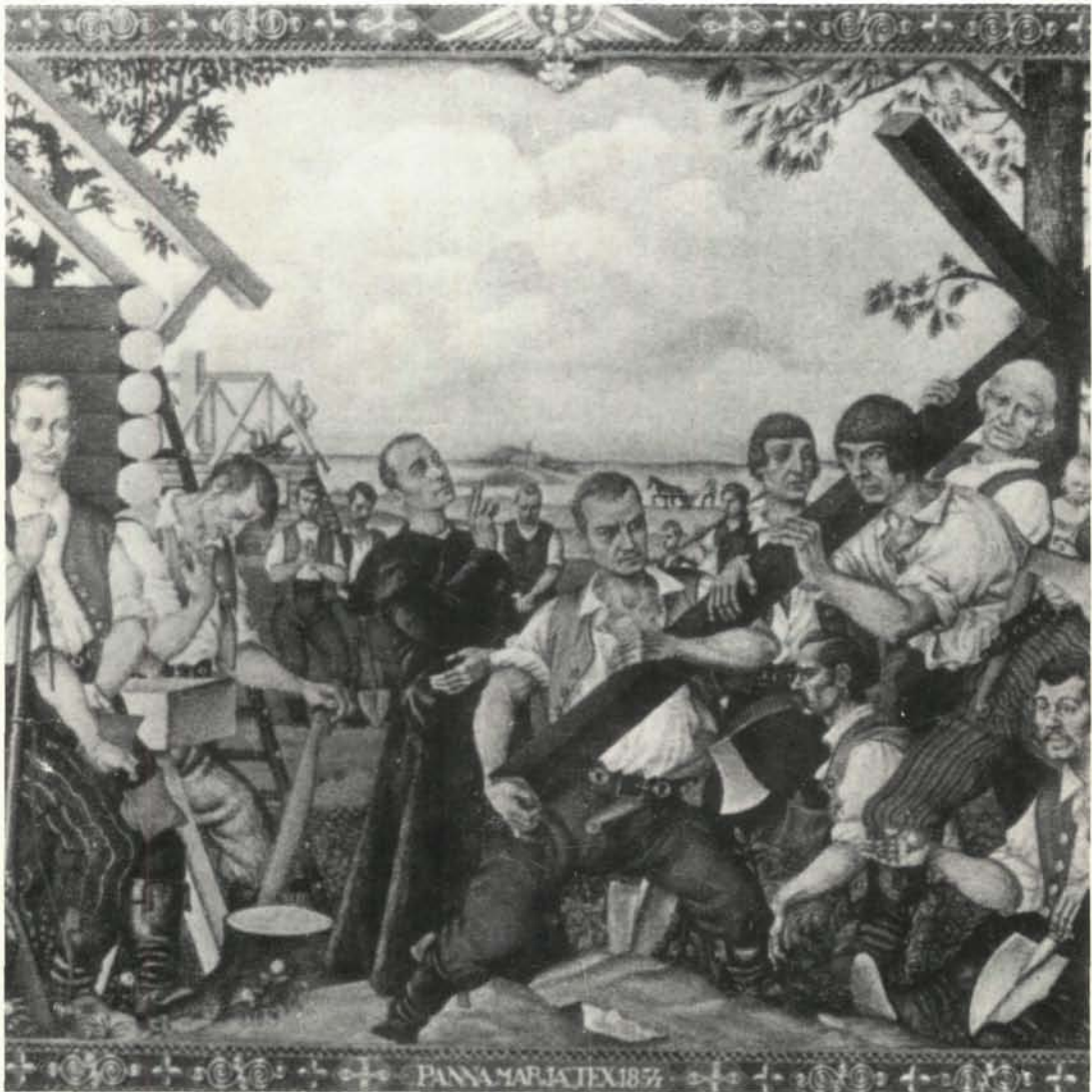


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TIMOTHY L. SMITH, professor of history and director of the Program in American Religious History at The Johns Hopkins University, is the author of *Revivalism and Social Reform in Mid-Nineteenth-Century America* (Nashville, 1957). A specialist in the social history of Christianity, he began his training under Charles J. Bishko at the University of Virginia and completed his doctoral work at Harvard University under the guidance of Arthur M. Schlesinger, Sr. His interest in ethnic studies dates from his years at the University of Minnesota, where he established the archives and research program now known as the Immigration History Research Center. He is currently completing a volume of essays on education, religion, and community in the United States and doing research for another on biblical notions of righteousness in American culture from the eighteenth century to the present.

ERRATA:

I wish to acknowledge and apologize for an error in my review of Glenn Davis, *Childhood and History in America* (*AHR*, 82 [1977]: 1319–20). The first word on page 1320 should be “childhood,” not “society.”

I regret that I did not catch this error until after publication of the review.

ROBERT H. BREMNER
Ohio State University

In the notes on contributors in the June issue of the *Review* (*AHR*, 83 [1978]: iv), it is stated that I am currently engaged in an “extended study of American imperialism.” I am not.

JAMES A. FIELD, JR.
Swarthmore College

Our apologies to Glenn Davis, Robert H. Bremner, James A. Field, Jr., and our readers.

THE EDITORS

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Religion and Ethnicity in America

TIMOTHY L. SMITH

THAT RELIGION AND ETHNICITY ARE INTERTWINED in modern urban and industrial societies is obvious, but the nature of this relationship and how it developed is not yet clear.¹ Recent studies of the religious aspects of cultural and social systems, particularly by anthropologists, have not yet freed historians from traditional notions about religion and ethnicity. Historians continue to believe that ethnicity is a synonym for nationality and that the religious and ethnic sentiments of immigrant minorities are anachronisms that must give way to the processes of modernization and assimilation.²

Although most European languages make it possible to use one word for what in English requires two—"nation" and "people"—ethnicity and nationality are not the same thing. Nationality is established by citizenship. Such varied modern states as Great Britain, Spain, the United States, Brazil, Yugoslavia, the Soviet Union, China, Czechoslovakia, and South Africa have brought under one government several peoples. The sense of peoplehood, moreover, which I take to be the essence of ethnicity, and the social structures that sustain it may flourish without reference to political nationhood at all, as until recently was the case for Jews, Gypsies, Sikhs, and American blacks.³ In the late nineteenth century many observers expected the sense of nationhood to replace that of peoplehood, either through the "melting pot" as in the

This essay originated as one of six addresses on "The American Experience," delivered at the annual meeting of the American Historical Association in Washington, D.C., December 28–30, 1976 in celebration of the United States bicentennial. Three of these addresses—by Arthur Schlesinger, jr., Robert Kelley, and C. Vann Woodward—were published previously in the bicentennial issue of the *American Historical Review* in June 1977. THE EDITOR.

¹ Robert Kelley, "Ideology and Political Culture from Jefferson to Nixon," *AHR*, 82 (1977): 531–62. For two beginnings at clarification by sociologists, see Harold J. Abramson, *Ethnic Diversity in Catholic America* (New York, 1973); and Thomas F. O'Dea, *Sociology and the Study of Religion: Theory, Research, Interpretation* (New York, 1970).

² For a study that suffers from these faults but deals perceptively with the fallacies of the assimilationist model, see John Higham, *Send These to Me: Jews and Other Immigrants in Urban America* (New York, 1975), 9–20, 202–09, 234–35. But see the essays by Josef Barton, William Galush, and Robert Mirak in Randall Miller and Tom Marzik, eds., *Immigrants and Religion in Urban America* (Philadelphia, 1977), 3–24, 84–102, 138–60.

³ For an ethnographic survey of old and persisting identities, see John Geipel, *The Europeans: The People, Yesterday and Today—Their Origins and Interrelations* (New York, 1970). Meic Stephens has argued that only since about 1840 did language become "the symbol of nationality"; Stephens, *Linguistic Minorities in Western Europe* (Llandysul, Wales, 1976), xix–xx, *passim*. But see Pierre L. Van den Berghe, "Ethnic Pluralism in Industrial Societies: A Special Case?" *Ethnicity*, 3 (1976): 242–54.

United States or through the suppression of ethnic loyalties by such policies as Russification, Magyarization, and Germanization. Events have demonstrated, however, that ethnicity is not an anachronism. The mobilization of ethnic groups in modern nations, whether among immigrants such as Finns in the United States or East Indians in Guyana or among peoples who, as the Welsh in Britain or the Georgians in the Soviet Union, are still largely resident in their ancient homelands, has aimed at the future, not at the past.⁴ Whether in cities or agricultural regions, near their ancestral villages or in distant lands, modern ethnic movements function chiefly to protect or advance the economic, cultural, or religious interests of persons who, by reason of some combination of actual or supposed common origin, language, or faith, believe they constitute one people.⁵ In the process of mobilizing these movements, leaders have often manipulated for immediate purposes the symbols of old national allegiances or invoked the ideology of new ones and thereby made notable contributions to nationalist movements in their homelands. But ethnicity and nationality ought not to be confused. The sense of inherited or acquired identity of the majority people was only one of several sources of the sentiments that sustained nationhood in Europe. The purposes of ethnic organizations in the emigrant *diaspora*, moreover, were far broader than the promotion or protection of an Old World political ideal, even though names such as "Serb National Defense League" or "Polish National Alliance" adorned their office doors.⁶

Following Emile Durkheim and Max Weber, sociologists have long stressed the interrelated functions of religion and ethnicity but, until recently, have also tended to regard both as artifacts of an outmoded past.⁷ The title of Milton Gordon's instructive volume on "the role of race, religion, and national origins" in the United States conveyed both a conclusion and a prediction: *Assimilation in American Life*. Much of the point of Nathan Glazer's early contribution to the sociological analysis of ethnicity lay in his surprise—

⁴ For historical works that make this point, see A. William Hoggland, *Finnish Immigrants in America, 1880-1920* (Madison, Wisc., 1960); and Josef J. Barton, *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City* (Cambridge, Mass., 1975). Also see David Lowenthal, *West Indian Societies* (New York, 1972), 144-77, 210-11; and Michael Hechter, "The Political Economy of Ethnic Change," *American Journal of Sociology*, 79 (1974): 1152-56, and *Internal Colonialism: The Celtic Fringe in British National Development* (Berkeley and Los Angeles, 1975), esp. 311-40.

⁵ See the Introduction to Nathan Glazer and Daniel P. Moynihan, eds., *Ethnicity: Theory and Practice* (Cambridge, Mass., 1975), 18, 25. Paul R. Brass has described North Indian processes of ethnic-group formation out of varied kinds and degrees of shared economic interests, religion, and other cultural resources; Brass, "Ethnicity and Nationality Formation," *Ethnicity*, 3 (1976): 225-31. And Charles F. Keyes has argued that belief in shared descent is a crucial but often contrived aspect of functioning ethnicity; Keyes, "Towards a New Formulation of the Concept Ethnic Group," *Ethnicity*, 3 (1976): 202-06.

⁶ Thomas N. Brown, *Irish-American Nationalism, 1870-1890* (Philadelphia, 1966), 85-130; Joseph Rothchild, *East Central Europe between the Two World Wars* (Seattle, 1974), 83-84; Mark Stolarik, "Immigration and Eastern Slovak Nationalism," *Slovakia*, 26 (1976): 13-20; and William Galush, "American Poles and the New Poland: An Example of Change in Ethnic Orientation," *Ethnicity*, 1 (1974): 216-19.

⁷ Weber, "Ethnic Groups," in Talcott Parsons et al., eds., *Theories of Society*, 1 (Glencoe, Ill., 1961): 305-09; and Durkheim, *On Morality and Society: Selected Writings*, ed. Robert N. Bellah (Chicago, 1973), ix-x, 169, 222-23. Also see W. Lloyd Warner and Leo Srole, *The Social Systems of American Ethnic Groups* (New Haven, 1945), 156-61, 283-96; and Will Herberg, *Protestant-Catholic-Jew: An Essay in Religious Sociology* (rev. ed., New York, 1960).

shared by the academic community generally—that it had persisted.⁸ In the late 1960s a flood of new sociological studies assumed the instrumental and, hence, plastic nature of ethnic affiliation and identity and, in a few cases, analyzed the interweaving of religious ideas, customs, and institutions with the choices and the chances of free persons.⁹ Thomas F. O'Dea has recently revived Weber's use of the example of ancient Judaism (in contrast to Durkheim's use of Australian totemism) to demonstrate that systems of religious thought not only may serve to legitimize existing social arrangements but, through prophetic proclamation in a time of crisis, can help break the chains of custom by making new and revolutionary demands, dissolving myths, and declaring a transcendent ethic not identifiable with any existing society or social institution.¹⁰

The resurgence of ethnicity as a factor in United States politics during the 1960s—involving first blacks, then Chicanos, Puerto Ricans, American Indians, Orientals, and “white ethnics”—was only one manifestation, as British sociologist Michael Hechter put it, of a “resurgence of ethnic political conflict in the most highly differentiated societies.” This resurgence challenged the prediction of Max Weber and Talcott Parsons that urbanization and industrialization would gradually erode belief in the sacredness of the “primordial” ethnic sentiments that flourish in “relatively undifferentiated social settings,” making way for the formation of political associations by “individuals of similar market orientations.” Hechter pointed out that the opposite has been happening: political movements in many advanced societies champion minority languages, promote “national” cultures, and generally seek to “legitimate new cultural forms in the guise of old ones.”¹¹

Certainly, the worldwide politics of peoplehood helped revive scholarly interest in the history of ethnicity in the United States.¹² My study of a pluralistic society in the Minnesota iron-mining country, begun with Clarke Chambers and Hyman Berman in 1962, yielded results that seemed to contradict prevailing historical dogma in complex ways and led us at first to the

⁸ Gordon, *Assimilation in American Life: The Role of Race, Religion, and National Origins* (New York, 1964); Glazer and Daniel P. Moynihan, *Beyond the Melting Pot: The Negroes, Puerto Ricans, Jews, Italians, and Irish of New York City* (Cambridge, Mass., 1963); and Glazer, *American Judaism* (Chicago, 1957), 79–126.

⁹ For key essays by many of the sociologists who produced these studies, see Glazer and Moynihan, *Ethnicity: Theory and Practice*; especially see, for the choices of the Chinese in Guyana and Jamaica to convert to the Anglican and Roman Catholic faiths, Donald L. Horowitz, “Ethnic Identity,” and Orlando Patterson, “Context and Choice in Ethnic Allegiance: A Theoretical Framework and Caribbean Case Study,” in *Ethnicity: Theory and Practice*, 114–18, 305–36. But see Harold J. Abramson, “On the Sociology of Ethnicity and Social Change: A Model of Rootedness and Rootlessness,” *Economic and Social Review*, 8 (1976): 43–48.

¹⁰ O'Dea, “Stability and Change and the Dual Role of Religion,” in Bernard Barber and Alex Inkeles, eds., *Stability and Social Change* (Boston, 1971), 161–65, 172–77.

¹¹ Hechter, *Internal Colonialism: The Celtic Fringe in British National Development*, 312–14.

¹² This interest has been evident in the renewed vitality of the Immigration History Society, in the establishment of several university research programs (notably the University of Minnesota's Immigration History Research Center), and in the launching of the journal *Ethnicity*. For additional evidence, see Andrew K. Greeley, *Ethnicity in the United States: A Preliminary Reconnaissance* (New York, 1974); and George Brown Tindall, *The Ethnic Southerners* (Baton Rouge, 1976). Rudolf J. Vecoli has called for more attention to the subject; Vecoli, “Ethnicity: A Neglected Dimension of American History,” in Herbert J. Bass, ed., *The State of American History* (Chicago, 1970), 70–89.

notion that developments there might not be typical. Two subsequent research projects and the work of a dozen graduate students have convinced me that the use of churches and synagogues to promote education and upward mobility as well as to define, rationalize, and revitalize ethnoreligious identity was not unusual but rather characteristic of the urbanization of both American blacks and migrating East European villagers, whether in cities near their birthplaces or in Rochester, Cleveland, Chicago, and Minneapolis.¹³

In light of this research America's "urban villagers," to use Herbert Gans's phrase, did not appear as resistant to change as Gans and others thought them to be, either in their new environment or in the village settings from which they came. John Briggs and Josef Barton have now offered persuasive evidence that rural villages in nineteenth-century Sicily, Calabria, and Transylvania underwent those social changes that are often labeled "modernization" or, perhaps less arguably, "urbanization" long before large numbers of their younger residents began to migrate to cities near or far away. With others I have concluded that such modernization also occurred in the Danube basin north of Belgrade, in the Austrian provinces of Slovenia, Galicia, and Bukovina, in Lithuania and Finland, and in that area that is now Eastern Slovakia and the sub-Carpathian Ukraine. Where these changes took place amidst a long-existing cultural diversity, the intertwining of religious feelings with ethnic interests and identities gave both to faith and to the sense of peoplehood a fluid and instrumental quality that was more future-oriented than backward-looking. Emigrants to the United States regrouped on this side of the Atlantic into larger aggregations that both preserved and revised inherited patterns of language, religion, and regional culture. These changes thus demonstrate a dynamic relationship between religion and ethnicity, not the static one that was long the model historians and sociologists followed.¹⁴

Anthropologists, who came late to the game of modern ethnic studies, seem at the moment to be leading it, in part because they have never discounted the functional significance of religion in culture. True, the first generation, whose

¹³ I must here acknowledge my extensive debts to a group of student colleagues who have steadily shared their findings with me for a decade: Josef Barton, John Briggs, William Galush, Mark Stolarik, Matt Susel, Paula Benkart, Arunas Alisauskas, and Frederick Hale; most of their studies are cited below. For a brief report with little reference to the role of religion, to which I subsequently turned, see my "New Approaches to the Study of Immigration in Twentieth-Century America," *AHR*, 71 (1965-66): 1265-79. For my recent review of these issues, see "Native Blacks and Foreign Whites: Varying Responses to Educational Opportunity in America, 1880-1950," *Perspectives in American History*, 6 (1972): 309-36. For recent works—in addition to Barton's *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City*—that confirm our group's general findings about social mobility and, therefore, diminish the cogency of Stephan Thernstrom's *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964), see Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880-1970* (Cambridge, Mass., 1973), 76-176; and Thomas Kessner, *The Golden Door: Italian and Jewish Immigrant Mobility in New York City, 1880-1915* (New York, 1977), esp. 165-70.

¹⁴ For two works that reflect essentially static views of southern Italian peasant culture, see Herbert Gans, *The Urban Villagers* (Glencoe, Ill., 1962); and Rudolf J. Vecoli, "Prelates and Peasants: Italian Immigrants and the Catholic Church," *Journal of Social History*, 2 (1969): 227-33. In comparison, see Barton, *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City*, 27-47; John Walker Briggs, *An Italian Passage: Immigrants to Three American Cities, 1890-1930* (New Haven, 1978), chaps. 1-3; and Eric R. Wolf, *Peasants* (Englewood Cliffs, N.J., 1966), 83-84. And see my "Lay Initiative in the Religious Life of American Immigrants, 1880-1950," in Tamara K. Hareven, ed., *Anonymous Americans: Explorations in Nineteenth-Century Social History* (Englewood Cliffs, N.J., 1971), 214-49.

works historians know best, was preoccupied with premodern village or “tribal” societies, in which the whole population shared one language and faith. But anthropologists have closely observed the manner in which religious and communal rituals regulated behavior, legitimized power, transformed group memory into ideology, and gave social meaning to each stage in the cycle of individual lives. They have found that pervasive symbols and the recounting or re-enactment of traditional stories drew together the conceptions each people held of the actual and ideal worlds. Yet the preoccupation of anthropologists with such homogenous communities, usually at a given moment in time, provided little impulse to challenge assumptions about the essentially static character of religion in premodern societies.¹⁵

After World War II the organization of new nations in Africa, Asia, and the Caribbean requiring the political integration of disparate peoples posed questions that prompted a different theoretical approach. Clifford Geertz, who studied Javanese religious cultures, and J. C. Mitchell, who examined the shifting character of tribalism in urban Zaire, have stressed the conflict between such new “integrative” states as Indonesia and what Geertz has called the “enduring structure of primordial identifications” rooted in religious as well as other aspects of primitive societies. Geertz’s model and Harold J. Isaacs’s development of it in *Idols of the Tribe* have proved attractive to historians and sociologists, in part, perhaps, because Geertz and Isaacs have not seriously disturbed the notion that sentiments that sustain both religion and a sense of unique peoplehood are archaic, even though surprisingly resilient.¹⁶

Recently, Abner Cohen’s studies of African cities—initially of the Hausa community of Muslim cattle merchants in Ibadan, Nigeria—sparked a quantum leap in the sophistication of anthropological inquiry into ethnicity.¹⁷ Following his lead, scholars have examined the pragmatic and often protean uses of ethnicity in widely varying urban settings and have demonstrated that ethnic movements are often a political phenomenon—a mobilization of cultural resources, in which religion is sometimes central, to serve immediate “interests” or goals. These scholars have explicitly rejected the static “primordialist” dogma of Geertz and Isaacs at the very moment when some American historians were finding in it a comprehensive rationale for ethnic

¹⁵ For studies that depart from these assumptions, see Emerick K. Francis, *In Search of Utopia: The Mennonites in Manitoba* (Glencoe, Ill., 1955), 5–7, 15–39; Martin Halpern, *A Serbian Village* (New York, 1958), chaps. 2, 10; and John W. Cole and Eric R. Wolf, *The Hidden Frontier: Ecology and Ethnicity in an Alpine Valley* (New York, 1974). For comparison, see Clifford Geertz, *The Interpretation of Cultures: Selected Essays* (New York, 1973), 87–89, 126–29, 140–41. For a parallel definition of the basis of ethnicity with a psychoanalytic orientation, see Harold R. Isaacs, “Basic Group Identity: The Idols of the Tribe,” in Glazer and Moynihan, *Ethnicity: Theory and Practice*, 29–45.

¹⁶ Geertz, “The Integrative Revolution: Primordial Sentiments and Civil Politics in New States,” in Geertz, ed., *Old Societies and New States* (New York, 1963), 90–114, esp. 114; Mitchell, *The Kalela Dance: Aspects of Social Relationships among Urban Africans in Northern Rhodesia* (Atlantic Highlands, N.J., 1956); and Isaacs, *Idols of the Tribe: Group Identity and Political Change* (New York, 1975), 38–46, 144, 147–48, 150, 162–65. Also see Edward Shils, “Primordial, Personal, Social, and Civil Ties,” *British Journal of Sociology*, 8 (1957): 130–45; and Geertz, *The Religions of Java* (1960; reprint ed., Chicago, 1976), 1–15, 355–81.

¹⁷ Cohen, *Custom and Politics in Urban Africa: A Study of Hausa Migrants in Yoruba Towns* (Berkeley and Los Angeles, 1969), 190–94. For the religious revitalization of 1951–52, see *ibid.*, 149–60.

studies.¹⁸ Even those who found Cohen's stress upon the political functions of ethnicity too narrow for the cases they studied have emphasized that the boundaries of peoplehood were elastic, not fixed, and that a new religious commitment often marked an outsider's incorporation within a group.¹⁹

Along with these advances in anthropology, sociology, and social history, two other scholarly developments especially pertinent to the American experience prompt me to propose a new beginning. First, historians of Central and Eastern Europe, following the lead of Peter F. Sugar and Donald Treadgold, have elaborated the ethnoreligious diversities that underlay political change in the Habsburg and Russian empires and in the Balkans as Ottoman control receded. The pluralistic and dynamic character of the relationships among the varied peoples of these regions helps explain the easy acceptance of pluralism by their emigrant contingents in the United States.²⁰

Second, both Jewish and Christian scholars have promoted a broad renewal of interest in Old Testament theology, partly because of the retrospective impact of the Holocaust upon Jewish thought and partly because of the herculean research of William F. Albright and his students. This theological revival has emphasized Jahweh's covenant to sustain a particular people, formed by their acceptance of his law and lordship, in all the immense challenges of their centuries-long *diaspora*. Recent works such as Emil Fackenheim's *God's Presence in History*, which grounds the culture of Judaism in the Exodus, and Jürgen Moltmann's *Theology of Hope* have underscored the contribution of Jewish messianism and Christian millennialism to the idea of progress. This wide-ranging scholarly effort to make biblical sense of "the reality we all live in," to use Geertz's phrase, has demonstrated afresh that both Judaism and Christianity have aimed to hold together what Geertz calls "ethos," which he defines as a people's view of "right" ideals for life, and "world-view," by which he means their understanding of social reality. Furthermore, both faiths have sought this union of ethos and world-view not simply in the ritualized practices and symbolic behaviors that have pre-

¹⁸ Abner Cohen, ed., *Urban Ethnicity* (London, 1974), xii-xv, xxiii; David Parkin, "Congregational and Interpersonal Ideologies in Political Ethnicity," in Cohen, *Urban Ethnicity*, 119-27; and Edward Allworth, "Regeneration in Central Asia," in Allworth, ed., *The Nationality Question in Central Asia* (New York, 1973), 3-5. For the most incisive statement I have read of the political nature of ethnic mobilization, see Immanuel Wallerstein, "The Two Modes of Ethnic Consciousness: Soviet Central Asia in Transition?" in Allworth, *The Nationality Question in Central Asia*, 168-75. But see Judith Nagata, "What Is a Malay? Situational Selection of Ethnic Identity in a Plural Society," *American Ethnologist*, 1 (1974): 331-33, *passim*.

¹⁹ For a discussion of the role of Pentecostal sects in the formation of the "Maragoli" group in rural Uganda, see S. R. Charsley, "The Formation of Ethnic Groups," in Cohen, *Urban Ethnicity*, 338-39, 344-49, *passim*. Also see Shlomo Deshen, "Political Ethnicity and Cultural Ethnicity in Israel during the 1960's," in Cohen, *Urban Ethnicity*, 282-85, 294-97, 302-06; and see Frederik Barth, Introduction, in Barth, ed., *Ethnic Groups and Boundaries* (Boston, 1970), 22-24, *passim*.

²⁰ Sugar, "The Nature of Non-Germanic Societies under Habsburg Rule," *Slavic Review*, 20 (1963): 16-24; Donald Treadgold, "The Peasant and Religion," in Wayne S. Vucinich, ed., *The Peasant in Nineteenth-Century Russia* (Stanford, Calif., 1965), 72-107; Emanuel Turczynski, "Nationalism and Religion in Eastern Europe," *East European Quarterly*, 5 (1972): 468-86; and Stavrou Skendi, *The Albanian National Awakening, 1878-1912* (Princeton, 1967). For a bibliographical account of recent interpretative trends, see R. V. Burns, *East European History: An Ethnic Approach*, American Historical Association Pamphlets, no. 425 (Washington, 1973).

occupied Geertz but in a set of remarkably persistent and rationally communicated ideas, to which the notion of a pilgrim people is central.²¹

Students of American ethnic history have scarcely begun to see the implications or to sift out and assimilate the results of this recent scholarship. The reconstruction of Jewish and Christian theology in terms of historical process is especially important to an analytical synthesis of the relationship between religion and ethnicity. That synthesis should concentrate upon immigrants from Europe because they came to this country in such numbers as to dominate the history of American ethnic pluralism. Although the injustices suffered by black, Chicano, Oriental, and American Indian minorities justify the multiplication of works on their sociology and history, the result is a growing cultural lag in scholarship. Recent studies of these "racial" minorities routinely analyze aspects of their ethnoreligious systems that sustain emerging social and political objectives, whereas scholars usually treat the religious institutions, cultural forms, and ideologies of immigrant Protestants, Jews, Catholics, and Eastern Orthodox Christians as backward-looking, dysfunctional, or arcane. This essay aims to redress the balance by focusing primarily on the role of religion among groups recently dubbed "white ethnics" and by pointing out resemblances to the way churches have sustained black and Hispanic-American ethnicity.

THE MOBILIZATION OF WHAT BECAME AMERICA'S IMMIGRANT PEOPLES began in most instances in their homelands, amidst a complex rivalry for economic and cultural advantage. Even in the Old World, the developing sense of peoplehood depended heavily upon religious identification, in some cases more so than upon language or myths about common descent. Migration to America, both before and after the United States became a largely urban and industrial society, produced three important alterations in the relationship of faith to ethnic identity: (1) a redefinition, usually in religious terms, of the boundaries of peoplehood, bringing folk memories to bear upon new aspirations; (2) an intensification of the psychic basis of theological reflection and ethnoreligious commitment, due to the emotional consequences of uprooting and repeated resettlement; and (3) a revitalization of the conviction, whether from Jewish messianism or from Christian millennialism, that the goal of history is the creation of a common humanity, a brotherhood of faith and faithfulness. The last two developments made the relationship between religion and ethnicity dialectical. Even while affirming that the unity of all mankind was the goal, intensified religious commitment defined more sharply the boundaries of subcultures and communities. In Western societies, both the confessional

²¹ Fackenheim, *God's Presence in History: Jewish Affirmations and Philosophical Reflections* (New York, 1970), chaps. 1, 3; and Moltmann, *Theology of Hope, on the Ground of the Implications of a Christian Eschatology* (New York, 1967). Also see Eric Voegelin, *Order and History*, vol. 1: *Israel and Revelation* (Baton Rouge, 1956), ix, xi, 126-27; and Geertz, *Interpretation of Cultures: Selected Essays*, 87-89, 139-41, 126-27. For an opposite argument that rites, not beliefs, are central to a religion's social role, see A. R. Radcliffe-Brown, *Structure and Function in Primitive Society: Essays and Addresses* (New York, 1965), 153-65.

state churches and some of the most intensely pietistic and sectarian groups in each of the major traditions—Catholic, Orthodox, Jewish, and Protestant—have played a unifying role, even while ministering to regional, doctrinal, and ethnic divisions. How this was so in American immigrant communities and how it fostered that peculiar unity of opposites which John Higham has recently labeled “pluralistic integration” urgently needs clarification.²² This essay seeks to illuminate these three fundamental alterations in the relationship of faith to ethnicity in the United States by illustrating their pervasiveness and significance. Since the alterations began in Europe, I must begin by summarizing the religious aspects of the changes in culture, society, and ideology that took place there.

Long before the first substantial migrations of any people occurred, a mobile minority composed of clergymen, peddlers, fishermen, government officials, and soldiers had already sensed that they were part of a cultural community extending beyond their native villages and valleys. As the agricultural and commercial revolutions spread, moreover, “modern” ideas about personal success, individual autonomy, work, and risk taking penetrated even the most isolated European and Near Eastern villages. Conscription to the armed services took young men to distant places for short periods; agricultural laborers journeyed from the mountains to the plains at harvest time; and the expansion of trade first encouraged, then undermined, household industry. For decades, peddlers carted the new manufactured products across great distances. During the nineteenth century the railroads turned this trickle of goods into a torrent. The construction of railroads by wage labor recruited from villages where no one had ever seen a steam engine transformed rural economies and peasant perceptions of time and space. Meanwhile, the population explosion that began in the seventeenth century had prompted the migration of peoples such as the Scots, Germans, Jews, Rusins, and Rumanians into nearby lands and villages. The resulting conflict and accommodation among groups intensified the sense of uniqueness and sharpened the psychic boundaries that language, culture, and religion drew among peoples who lived close together.²³

Ethnoreligious diversity characterized many of the areas of Western Europe that first sent peoples to America. Colonial Englishmen were highly conscious of the religious and other cultural characteristics that distinguished them from Scottish, Welsh, and Scotch-Irish Protestants and especially from the Catholic Irish, whose migration to both English and American towns began

²² John Higham, “Hanging Together: Divergent Unities in American History,” *JAH*, 41 (1974): 5–28, foreshadowed in its title a central point in his *Send These to Me: Jews and Other Immigrants in Urban America*, 240–42.

²³ Ian Charles C. Graham, *Colonists from Scotland: Emigration to North America, 1707–1783* (Ithaca, N.Y., 1956) chaps. 1–3; Mack Walker, *Germany and the Emigration, 1816–1885* (Cambridge, 1965), 1–9; Laurence J. McCaffrey, *The Irish Diaspora in America* (Bloomington, Ind., 1976), 60–61; Carlile A. Macartney, *Hungary and Her Successors: The Treaty of Trianon and Its Consequences, 1918–1937* (London, 1937), 83–94, 200–12, 262–75, 356–62, 380–90; Sugar, “Nature of Non-Germanic Societies under Habsburg Rule,” 16–24; and Gunther E. Rothenburg, “The Croatian Military Border and the Rise of Yugoslav Nationalism,” *Slavic and Eastern European Review*, 43 (1964): 35–38, 42.

in the middle of the eighteenth century. Northern Ireland, source of both Catholic and Protestant immigrants, gave colonial America a Scotch-Irish Puritanism quite different from the English variety dominant east of the Hudson River.²⁴ German-speaking migrants from the Rhineland represented not only various Protestant communions—from Mennonites and Brethren to Lutherans and Reformed Germans—but, as time passed, Catholic and Jewish populations as well. Both of the latter groups came in large numbers from Bavaria in the decades before the Civil War. The Netherlands, whose Protestant and Catholic divisions were reflected in the earliest settlers of New Amsterdam, sent some Quakers and Mennonites and a large Dutch Reformed contingent to the Middle Colonies and, later, both Reformed Dutch and Catholic Belgians to the Midwest.²⁵ Laplanders—some by language Swedish and others Finnish—were, like the Swede-Finns from certain Baltic islands, Lutheran in religion. In the decades preceding mass migration to America, all of the Scandinavian peoples experienced extensive religious awakenings that heightened their sense of identity as Danes, Norwegians, Swedes, or Finns.²⁶

Population expansion, resettlement of peoples, and intensified ethnic rivalry also characterized Central and Eastern Europe in the eighteenth and nineteenth centuries. Rumanians, Orthodox in religion and speaking a language rooted in both Slavic and Latin, spread northward over the Transylvanian plateau, complicating the cultural map of a region long occupied by German-speaking Saxons, Magyars, and an older native population whose ethnic and linguistic identity remains unresolved. Meanwhile, contingents of Carpathian and Galician Jews moved southward into Transylvania, while others fanned out into what is now Slovakia, Hungary, and Yugoslavia. In all of these regions, competition among peasants, tradesmen, and laborers for land and other economic advantages intensified the group loyalties originally defined by religion. Rumanian Orthodox chapels and fortress-like Saxon Lutheran churches still stand in separate sections of hundreds of Transylvanian villages; frequently, the same villages contain Jewish, Rumanian Greek Catholic, and (westward toward Hungary) Magyar Roman Catholic houses of

²⁴ On the general background, see Geipel, *The Europeans: The People, Today and Yesterday*. For Switzerland and Belgium, see William Petersen, "On the Subnations of Western Europe," in Glazer and Moynihan, *Ethnicity: Theory and Practice*, 177–208. R. J. Dickson has demonstrated that economic and not religious considerations provided the chief motive for Scotch-Irish emigration, but he has ignored the role of Presbyterian piety in mobilizing and sustaining their venture; Dickson, *Ulster Emigration to Colonial America, 1718–1775* (London, 1966), 24–25, 33–39. Also see Leonard J. Trinterud, *The Forming of an American Tradition: A Re-examination of Colonial Presbyterianism* (Philadelphia, 1949), 15, 30–31, 34–35, 71–72, 127–30, 137–38, 222–27, 261–64; and L. C. Rudolph, *Hoosier Zion: A Study of Presbyterianism in Indiana to 1850* (New Haven, 1963), 118–29.

²⁵ For careful essays on Lutheran, Reformed, Mennonite, Moravian, and Brethren immigrants, see F. Ernest Stoeffler, ed., *Continental Pietism and Early American Christianity* (Grand Rapids, 1976). Also see Henry S. Lucas, *Netherlanders in America: Dutch Immigration to the United States and Canada, 1789–1950* (Ann Arbor, 1955); and Eric Hirshler, ed., *Jews from Germany in the United States* (New York, 1955).

²⁶ Uuras Saarnivaara, *The History of the Laestadian or Apostolic-Lutheran Movement in America* (Ironwood, Mich., 1947); Florence E. Janson, *The Background of Swedish Immigration, 1840–1930* (Philadelphia, 1931), 167–221; and Einar Molland, *Church Life in Norway, 1800–1950*, trans. Harris Kaasa (Minneapolis, 1957), 2–3, 15–19, 33–41, 48–52.

worship. A similar pluralism emerged in the villages and towns of the great basin of the Danube north of Belgrade, called the Banat and the Batchka, and in what is today eastern Slovakia. In the larger Slovak towns, a small German Lutheran population (dating from the seventeenth and eighteenth centuries) profited from the special favor of the Habsburg monarchs. A strong educational program and the impulse to get ahead that migrants generally display enabled them to compete effectively with the far more numerous Slovak Roman Catholics. During the eighteenth and nineteenth centuries a steady stream of Rusin Greek Catholics poured from the Carpathian highlands into the vacant lands around the East Slovak villages; and into the villages and towns came Jews from the same area and Protestant and Roman Catholic Magyars from the south.²⁷

Elsewhere in Eastern Europe not only the towns but many of the villages were cockpits of cultural, economic, and political rivalry among ethnoreligious groups long before mass migration to America began. In the Austrian province of Galicia, now southern Poland, Roman Catholic Poles, Greek Catholic or Orthodox Rusins (or Ukrainians, as some of them came to prefer to be called), and Orthodox and Hasidic Jews lived together uneasily in the same towns and villages. In the nineteenth century the government in Vienna counteracted Polish agitation for independence by supporting the communal enterprises of the non-Polish peoples. For similar reasons, the Prussian government usually supported German interests against the Polish population in Posen, Upper Silesia, and the province of West Prussia. Eastward in Bukovina and Lithuania, as in all of the long borderland from the Baltic to the Bosphorus, the convergence of Catholic, Orthodox, and Protestant peoples and the rapid expansion of the Jewish population created similar rivalries. The tsarist government, insecure in its control over the western provinces, sought stability through Russification. Along the Dalmatian coast from Trieste to Dubrovnik, Catholic Slovenes and Croats and Orthodox Serbs likewise often lived side by side. Their exposure to Italian and Greek influences through the Adriatic ports as well as their proximity to the large Muslim populations in Bosnia and Herzegovina made the coastal as well as many inland towns and villages arenas of competing cultures. In all of these regions, economic rivalries accentuated the divisions that religion and language defined, as the

²⁷ For relevant sources on village backgrounds, see the citations in my "Lay Initiative in the Religious Life of American Immigrants, 1880-1950," 216-25. For the multi-ethnic character of village life, see Barton, *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City*, 27-47; Macartney, *Hungary and Her Successors: The Treaty of Trianon and Its Consequences, 1918-1937*, 75-94, 200-11, 251-74, 383-90; Oscar Jászi, *The Dissolution of the Habsburg Empire* (Chicago, 1929), 391-93; Jean Mousset, *Les villes de la Russie subcarpatique (1918-1938): L'effort Tchecoslovaque* (Paris, 1938), 17-26; David Friedmann, *Geschichte der Juden in Hunenné vom 13. Jahrhundert bis auf die Gegenwart* (Beregsas, Hungary, 1933), 48-74; Marion Mark Stolarik, "Immigration and Urbanization: The Slovak Experience, 1870-1918" (Ph.D. dissertation, University of Minnesota, 1974), 1-54; and Paula Kaye Benkart, "Religion, Family, and Community among Hungarians Migrating to American Cities, 1880-1930" (Ph.D. dissertation, Johns Hopkins University, 1975), 1-58. For the study that supersedes other discussions of the general question, see Turczynski, "Nationalism and Religion in Eastern Europe," 468-71, 475-76, *passim*.

various peoples competed for possession of the soil and for commercial as well as agrarian markets.²⁸

The ethnic mobilization of what became America's immigrant peoples began, then, in their homelands amidst complex economic and cultural rivalries. In each case the developing sense of peoplehood depended heavily upon a revitalization of religious faith and commitment. Religious awakenings helped define both the boundaries and the moral ideals of ethnic groups and thrust both townsmen and peasants toward the "modern" goals of autonomy, self-realization, and mobility that were crucial in the decision to migrate. The Irish national awakening, for example, may have helped generate and certainly drew inspiration from the "devotional revolution" which swept through Ireland and its emigrant colonies in England and America in the mid-nineteenth century. John Livingston's revivals, which "Presbyterianized" northern Ireland in the seventeenth century, had an effect upon the Scotch-Irish similar to that of the Wesleyan revival in England and the Pietist movements in Germany in the eighteenth.²⁹ The awakening of Serbian Orthodoxy—through which Danilo Jakšić, Bishop of Karlovac, marshaled cultural resources for the Serbian resurgence in the Croatian borderlands—roughly paralleled the invigoration of Carpathian Jewry by the intensely personal and communal piety of Hasidism. In the nineteenth century the Haugean and Johnsonian revivals among Norwegian Lutherans, the Ukrainian national movement that the Metropolitans of Lvov helped inspire, and the Slovak Catholic resistance to Magyarization also mobilized religious sentiments to serve ethnic purposes.³⁰

Moralism, rooted in biblical teachings but made intensely personal in religious revivals, was an essential ingredient of modernization. The fashioning of "new persons"—involving new perceptions of individual worth, enlarged hopes for both this life and the next, and the internalization of moral

²⁸ For a comprehensive and thoughtful analysis of the ethnoreligious situation in Austrian Galicia and Russian Poland, see Keith Dyrud, "The Rusin Question in Eastern Europe and in America, 1890–World War I" (Ph.D. dissertation, University of Minnesota, 1976), 16–74 (forthcoming, Arno Press). Also see William John Galush, "Forming Polonia: A Study of Four Polish-American Communities, 1890–1940" (Ph.D. dissertation, University of Minnesota, 1975), 1–44; Erich Prokopowitsch, *Die Romanische Nationalbewegung in der Bukowina und der Dako-Romanismus*, Studien zur Geschichte der Österreichisch-Ungarischen Monarchie, no. 3 (Graz, Austria, 1965), 35–110; Ludvik Nemec, "The Ruthenian Uniate Church in Its Historical Perspective," *Church History*, 37 (1969): 371–86; Macartney, *Hungary and Her Successors: The Treaty of Trianon and Its Consequences, 1918–1937*, 356–62; and Smith, "Lay Initiative in the Religious Life of American Immigrants, 1880–1950," 214–49. Arunas Alisauskas has research in progress on Lithuanians in Europe and America.

²⁹ Emmet Larkin, "The Devotional Revolution in Ireland, 1850–1875," *AHR*, 77 (1972): 636–52; Trinterud, *Colonial Presbyterianism*, 170; Bernard Semmel, *The Methodist Revolution* (New York, 1973), 3–40, 81–108; and Donald F. Durnbaugh, "The Brethren in Early American Church Life," in Stoeffler, *Continental Pietism and Early American Christianity*, 222–32. For an absorbing commentary on the issues Larkin has raised, see David W. Miller, "Irish Catholicism and the Great Famine," *Journal of Social History*, 9 (1975): 81–98.

³⁰ Turczynski, "Nationalism and Religion in Eastern Europe," 473–76; Gershom Scholem, *Major Trends in Jewish Mysticism* (New York, 1961), 325–50; Molland, *Church Life in Norway, 1800–1950*, 10, 19; Ivan L. Rudnytsky, "The Role of the Ukraine in Modern History," *Slavic Review*, 22 (1963): 203–15; and A. Hlinka, "The Influence of Religion and Catholicism on States and Individuals," in R. W. Seton-Watson, ed., *Slovakia Then and Now: A Political Survey* (London, 1931), 168.

constraints calculated to help converts realize these hopes—seems to have been the primary aim of all religious awakenings. Bernard Semmel has recently argued that John Wesley's lifelong battle against the "antinomian tendencies" of the Reformation doctrines of predestination and of justification "by faith alone" and his immensely complex recasting of Anglican, Arminian, and Moravian teachings stemmed from Wesley's conviction that biblical faith aimed at "holiness"—the moral regeneration of both individuals and societies. Earlier, George M. Stephenson and Einar Molland pointed out that Methodist moralism was critical in early nineteenth-century Swedish and Norwegian revivals, even though Wesley's sect itself won few adherents.³¹ The timing and indigenous character of European movements for temperance or total abstinence from alcoholic beverages in the nineteenth century is one clue to the nature of the revitalizations that took place.³²

Regional religious organizations both sustained and restricted cultural awakenings. In Central and Eastern Europe, for example, long before other agencies of modernization—railroads, newspapers, industrial employment, and the like—began deeply to affect group consciousness in isolated villages, the various religious communities—Roman and Greek Catholic, Lutheran, Reformed, Eastern Orthodox, and Jewish—had fashioned hierarchies of communication or authority that sustained the efforts of pastors, priests, rabbis, and lay leaders to help the members of local congregations find in their faith the moral resources to take advantage of a world of enlarging opportunities. By the nineteenth century, these competitive ethnoreligious structures functioned much like American denominations and were the administrative units with which national or imperial governments dealt. In multi-ethnic regions, a single parish or synagogue often served a half-dozen or more villages, many of which contained sizable populations of other faiths. The pastor or rabbi moved from day to day and week to week across his broad territory, somewhat like an American circuit rider, attending to the needs of groups that in some villages were too small to meet anywhere but in homes. Nineteenth-century Lutheran bishops made regular official visits to Slovak congregations in the northern counties of the old Kingdom of Hungary, for example, as did Reformed bishops from Debrecen to their Magyar congregations in Slovakia and Transylvania. Both were concerned for the preservation of the language as well as the faith of these ethnoreligious communities and for the education as well as the economic advancement of the children. The structure of Jewish life, particularly of Hasidism, was too congregational to allow a formal hierarchy to develop. Yet certain rabbis or zaddikim were most frequently consulted on questions of behavior or belief, and all Jewish congregations

³¹ Semmel, *Methodist Revolution*, 93–108, 191–98; and Stephenson, *The Religious Aspects of Swedish Immigration: A Study of Immigrant Churches* (Minneapolis, 1932), 116–17. Molland has noted the work ethic and the moral ideal of holiness in the Haugean reaction against Pietism; *Church Life in Norway, 1800–1950*, 10–19.

³² Joan Bland has recounted the indigenous origins and convergence during the 1840s of Irish temperance movements in the United States and Ireland; Bland, *Hibernian Crusade: The Story of the Catholic Total Abstinence Union of America* (Washington, 1951), 9–17, 21–24. Also see Janson, *The Background of Swedish Immigration, 1840–1930*, 172–76. For an influential exposition of the term "revitalization," see Anthony F. Wallace, *The Death and Rebirth of the Seneca* (New York, 1969), vii, 239–340.

were preoccupied with the social welfare of their members. As a consequence, in many regions Jewish religious communities became as “denominational” as those of Protestant, Catholic, Orthodox, and Greek Catholic Christians.³³

Through this variety of cultural awakenings, then, ethnic consciousness fortified by religious faith took hold of the imagination of many Europeans. It affected most the younger people, especially those with the earliest and best opportunities for schooling—precisely the group that provided the majority of emigrants to the United States. By the time they began to consider the possibility of migration, the interweaving of religious and ethnic feelings had become for many a deep-seated habit of mind.³⁴

Anticlericalism also flourished in nineteenth-century Europe. Some of the most socially progressive emigrants were hostile to doctrines and rites that they felt impeded progress. But they were a minority. The great majority of Greeks, Slovaks, Swedes, Magyars, Lithuanians, and Rusins that settled in America perceived their pastors, priests, or rabbis as agents of progress.³⁵ In both the Old World and the New, clergymen provided moral guidance and spiritual comfort to families unable to sustain themselves on the land and dismayed by the manifold adjustments to a commercial or industrial economy. Pastors served as “spiritual advisers” to the many types of mutual benefit societies that spread through nineteenth-century Scandinavia, Germany, Sicily, Calabria, and the Habsburg Empire. They not only legitimized but sometimes, as in the case of the Norwegian moral revivalist Hans Nielsen Hauge, originated plans for the reorganization of economic life. Clergymen were interested in the social welfare of peasants, craftsmen, shopkeepers, and fishermen and served as spokesmen for ethnic interests that the policies of the English, German, Austro-Hungarian, and Russian governments seemed to threaten. As a consequence, many clergymen performed a role in the mobilization of cultural resources that married ethnicity to religion on clearly “modern” terms.³⁶ Such a role seemed even more appropriate in America,

³³ For the “denominational” structure of religious life in eighteenth-century America, see my “Congregation, State, and Denomination: The Forming of the American Religious Structure,” *William and Mary Quarterly*, 3d ser., 25 (1968): 168–75. For nineteenth-century developments in Eastern Europe, see my “Lay Initiative in the Religious Life of American Immigrants, 1880–1950,” 214–49. Also see Turczynski, “Nationalism and Religion in Eastern Europe,” 468–75; and Scholem, *Major Trends in Jewish Mysticism*, 336–37, 344–47.

³⁴ Abramson has argued that “societal competition” among differing faiths was indispensable to the development of ethnoreligious identity; see *Ethnic Diversity in Catholic America*, 127–52.

³⁵ Timothy L. Smith, “Religious Denominations as Ethnic Communities: A Regional Case Study,” *Church History*, 35 (1966): 207–26; Mary B. Trendley, “Formal Organization and the Americanization Process, with Special Reference to the Greeks of Boston,” *American Sociological Review*, 14 (1949): 44–53; and Dyrud, “The Rusin Question in Eastern Europe and in the United States, 1890–World War I,” 1833–89.

³⁶ Their role in mobilizing ethnic groups seems to fit exactly what Glazer and Moynihan have posited as probably the most vital function of ethnic leadership; “Introduction,” *Ethnicity: Theory and Practice*, 18, 25. For a description of a comparable development in Ceylon in the 1950s, see Geertz, “Integrative Revolution: Primordial Sentiments and Civil Politics in New States,” 122. For pastoral examples in Europe, see Bland, *Hibernian Crusade: The Story of the Catholic Total Abstinence Union of America*, 22–23; Walter J. Kukkonen, “The Influence of the Revival Movements of Finland on the Finnish Lutheran Churches in America,” in Ralph J. Jalkanen, ed., *The Faith of the Finns: Historical Perspectives on the Finnish Lutheran Church in America* (East Lansing, Mich., 1972), 82–93; Stephenson, *Religious Aspects of Swedish Immigration*, 74–132, esp. 103–05; Molland, *Church Life in Norway, 1800–1950*, 10, 15–16; and Wilhelm Austerlitz, *Leben und Werken von Weitland Rabbi Dr. [Mayer] Austerlitz . . .* (Prešov, Slovakia, 1928), 8, 13, 15–16.

and the laymen who founded congregations here expected the clergymen whom they brought from Europe to perform it.³⁷

THUS, THE THREE MAJOR ALTERATIONS in the relationship between ethnicity and religion that took place in America extended and intensified what had begun in Europe. The first of these, to which I now turn, was the redefinition of ethnic boundaries in religious terms. This involved a broadening of the geographic and linguistic backgrounds of persons deemed suitable for inclusion and frequently a decisive narrowing of religious ones.

That this nation's ethnic groups, viewed structurally, were made in America by the voluntary association of newcomers has long been evident.³⁸ Less clear is the fact that the models for this development had emerged earlier in the multi-ethnic arenas of Europe. What in premodern societies had been the experience of wandering tradesmen, scholars, soldiers, government officials, and religious pilgrims—discovering far from their home villages persons similar to themselves in language, cult, or custom and accepting them as “brothers and sisters” of presumed common descent—became routine among migrants to America. Ethnic organizations coalesced out of both economic and psychic need and found meanings for personal and communal life in the cultural symbols and the religious ideas that their leaders believed were marks of a shared inheritance and, hence, of a common peoplehood. Both the structure and the culture of these emerging ethnoreligious groups helped participants compete more advantageously with members of other groups. And, once established, each group constituted—in Milton Gordon's classic terms—a social system in which the members could satisfy all of their needs for structured human relationships from the cradle to the grave.³⁹ The Ameri-

³⁷ For a penetrating revision of long-held views, see Josef J. Barton, “Religion and Cultural Change in Czech Immigrant Communities,” in Miller and Marzik, *Immigrants and Religion in Urban America*, 9–10, 14–15, 17. For other examples demonstrating that the transfer to America was often direct, see Arthur J. Goren, *New York Jews and the Quest for Community* (New York, 1970), 76–85; Stephenson, *Religious Aspects of Swedish Immigration*, 264–77, 384–94; Jay P. Dolan, *The Immigrant Church: New York's Irish and German Catholics, 1815–1865* (Baltimore, 1975), 64–66, 81–84, and *Catholic Revivalism in the United States, 1830–1900* (South Bend, Ind., 1978), 21–23, 35–36, 189–93; Bernard Coleman and Verona LaBud, *Masinaigans—The Little Book: A Biography of Monsignor Joseph F. Buh, Slovenian Missionary in America, 1864–1922* (St. Paul, 1972), 130–31, 184–87; Arlow W. Anderson, *The Norwegian-Americans* (Boston, 1975), 13–15, 95–98, 102–08; Gillian L. Gollin, *Moravians in Two Worlds: A Study of Changing Communities* (New York, 1967), 9–24, 165–216; Vladimir Kaye, *Early Ukrainian Settlements in Canada, 1895–1900: Dr. Joseph Oeske's Role in the Settlement of the Canadian Northwest* (Toronto, 1964); and John Walker Briggs, “Church Building in America: Divergent and Convergent Interests of Priests and Lay People in Italian-American Communities,” paper read at the Hopkins-Harwichport Seminar in American Religious History, 1975.

³⁸ See William I. Thomas and Florian Znaniecki, *The Polish Peasant in Europe and America* (Chicago, 1918); Oscar Handlin, *Boston's Immigrants: A Study in Acculturation* (Cambridge, Mass., 1941); Hoglund, *Finnish Immigrants in America, 1800–1920*; and Philip Gleason, *The Conservative Reformers: German-American Catholics and the Social Order* (Notre Dame, Ind., 1968). Also see Theodore C. Blegen, *Norwegian Migration to America: The American Transition* (Northfield, Minn., 1940); Theodore Saloutos, *The Greeks in the United States* (Cambridge, Mass., 1964); and Rudolph J. Vecoli, “European Americans: From Immigrants to Ethnic,” in William H. Cartwright and Richard L. Watson, Jr., eds., *The Reinterpretation of American History and Culture* (Washington, 1973), 81–112.

³⁹ Gordon, *Assimilation in American Life*, 34–51. For Gordon's more recent thinking, see his “Toward a General Theory of Racial and Ethnic Group Relations,” in Glazer and Moynihan, *Ethnicity: Theory and Practice*, 84–110.

can communities were not simply transplantings of Old World political and religious loyalties but reasoned efforts to deal with new challenges. That the national movements in Slovakia, Lithuania, and Poland drew heavily for inspiration, leadership, and funds upon their countrymen in the United States ought not to distort our understanding of the differing cultural, economic, and political purposes of the organizations that the immigrants founded here.⁴⁰

Formal affiliation, however, turned on personal choice. Ethnic association—here defined as residence in a boarding house or tenement, membership in a local or national mutual benefit society, or participation in a musical, dramatic, or recreational club—was determined largely by the immigrant's identification with a particular religious tradition.⁴¹ The appeal of common language, national feeling, and belief in a common descent was sufficient in only a few minor cases to outweigh the attraction of religious affiliation as an organizing impulse.⁴² Two problems of perception have obscured this fact: the preoccupation of historians and sociologists with the secular aspects of ethnicity and nationality;⁴³ and the unexamined assumption that the experiences of linguistic groups among whom no substantial religious divisions existed (Greeks, Poles, Italians, Slovenes, French-Canadians, Scots, and Chicanos) were typical of all groups. Scholars and journalists have written about a major religious segment of the German, Czech, Slovak, Hungarian, Arab, or Russian populations in America under the assumption that ethnic identity was defined by language, while largely ignoring another branch or branches of the same linguistic group whose ethnic life revolved around a different religious affiliation.⁴⁴

⁴⁰ Stolarik, "Immigration and Urbanization: The Slovak Experience, 1870-1918"; Galush, "Polish-American Communities, 1890-1940"; Dyrud, "The Rusin Question in Eastern Europe and in America, 1890-World War I"; Arunas Alisauskas, "Religion, Ethnicity, and the Emergence of a Lithuanian Subculture in the United States, 1870-1900," paper read at the Hopkins-Harwichport Seminar in American Religious History, August 1974; and Timothy L. Smith, "Immigrant Social Aspirations and American Education, 1880-1930," *American Quarterly*, 21 (1969): 539-42. Also see Victor Greene, *For God and Country: The Rise of Polish and Lithuanian Ethnic Consciousness in America, 1860-1910* (Madison, Wisc., 1975), 85-99, 143-53.

⁴¹ Several groups of graduate students and I have closely examined the marriage records of numerous immigrant congregations and the personnel records of the mutual benefit societies that supported them. We found that members of local ethnic communities almost invariably came from a wide variety of villages and usually from two or more regions in their homelands. Also see Vilho Niitemaa, "Emigration Research in Finland," in Michael G. Karni, Matti E. Kaups, and Douglas J. Ollila, Jr., eds., *The Finnish Experience in the Western Great Lakes Region: New Perspectives* (Turku, Finland, 1975), 31-33; Dolan, *Immigrant Church: New York's Irish and German Catholics, 1815-1865*, 72-73; and Walter O. Forster, *Zion on the Mississippi: The Settlement of the Saxon Lutherans in Missouri, 1839-1841* (St. Louis, 1953), 150.

⁴² For the rich variety of ethnoreligious organizations of Arabic-speaking Americans, see Barbara C. Aswad, ed., *Arabic-Speaking Communities in American Cities* (New York, 1974). For other examples in which language and ethnicity are not correlated, see Keyes, "Towards a New Formulation of the Concept Ethnic Group," 202-04; and Petersen, "Subnations of Western Europe," 177-79. Also see Dell H. Hymes, "Linguistic Problems in Defining the Concept of Tribe," in J. Helm, ed., *Essays on the Problem of Tribe* (Seattle, 1968), 23-48. For a theoretical reconstruction of the whole problem, see Dell H. Hymes, *Foundations in Sociolinguistics* (New York, 1974), 18-19, 30-32, 45-53, 102-05.

⁴³ This preoccupation is especially obvious in the essays in Glazer and Moynihan, *Ethnicity: Theory and Practice* and in the papers presented at the Schouler Lectures and Symposium on Ethnic Leadership at Johns Hopkins University in February 1976. Oscar and Mary F. Handlin and Thomas Kessner have, moreover, virtually ignored religion; Handlin and Handlin, "The New History and the Ethnic Factor in American Life," *Perspectives in American History*, 4 (1970): 21-23; and Kessner, *The Golden Door: Italian and Jewish Immigrant Mobility in New York City, 1880-1915*.

⁴⁴ For one of the finer recent studies, see Kathleen Conzen, *Immigrant Milwaukee, 1836-1860: Accommoda-*

By its policy since 1920 of recording the “mother tongue” of the population, the federal Census Bureau has encouraged the notion that language was the bench mark of ethnicity. For their own reasons, the editors of foreign language newspapers have for decades declared this to be the case. Yet they, at least, knew that Norwegian and Danish were scarcely two languages, that Croatian and Serbian were closely related South Slavic dialects, that literary Ukrainian and Slovak were created in the nineteenth century in order politically to unite regional groups with diverse dialects, and that the dialects of northern and southern Italians were at least as dissimilar as Swedish and Norwegian.⁴⁵

Consider, for example, the role of religion among Germans in America. Those who settled in colonial Pennsylvania were divided between the “church” party (Lutheran or Reformed), whose congregations also attracted settlers of those persuasions hailing from Switzerland or the Low Countries, and the “sectarian” groups which had Anabaptist or Pietist backgrounds. So sharp were the distinctions between these two general alignments and so insistent were the Mennonites, Moravians, and Dunkers upon their particular separateness that, if we stay with Gordon’s functional definition, colonial Pennsylvania was the home of a half-dozen German ethnic groups. In the nineteenth century, large-scale immigration from Germany produced several new ethnoreligious communities: a Protestant one, bounded by the Missouri and Wisconsin Lutheran Synods; a Jewish community, the leadership of which soon passed to the Reform rabbis; a small but influential community of freethinkers, united in the *Turnvereine*; and the German Catholics. The organizational center of the last was not a separate church because Roman Catholic doctrine and structure forbade sectarian divisions; so the numerous local and national German Catholic mutual aid societies and their umbrella organization, the *Central-Verein*, provided a surrogate church and nurtured their sense of separate peoplehood.⁴⁶ Although in the twentieth century all of the German ethnic groups responded in similar ways to the political crises that the two world wars created in their relationship to other Americans, their segregation in separate parishes, clubs, and neighborhoods as well as the separate paths they took toward accommodation with non-German Protestants, Catholics, and Jews made the notion of a common German “nationality” important only

tion and Community in a Frontier City (Cambridge, Mass., 1976). In the chapter on religious developments, Conzen has dealt with both Protestant and Catholic Germans, but not with Jews. The same is true of Carl Wittke’s chapter on Germans, which also emphasizes free thinkers; Wittke, *We Who Built America: The Saga of the Immigrant* (rev. ed., Cleveland, 1964), 186–256.

⁴⁵ Meic Stephens has covered some of these differences from the point of view of a Welsh nationalist who considers all Gaelic speakers as one people; see *Linguistic Minorities in Western Europe*, xix–xx, 1–5, 479–552.

⁴⁶ For the early years, see Julius F. Sachse, *The German Pietists of Provincial Pennsylvania, 1694–1708* (Philadelphia, 1895), and *The German Sectarians of Pennsylvania*, 2 vols. (Philadelphia, 1899–1900). For splendid summaries, see John R. Weinlick on Moravians, Donald Durnbaugh on the Brethren or Dunkers, and James R. Tanis on Reformed Pietism in Stoeffler, *Continental Pietism and Early American Christianity*, 123–63, 222–65, 34–73. For the nineteenth century, see Conzen, *Immigrant Milwaukee, 1836–1860*, 158–67; Carl E. Schneider, *The German Church on the American Frontier* (St. Louis, 1939); Colman J. Barry, *The Catholic Church and the German-Americans* (Milwaukee, 1953), and “German Catholics and the Nationality Controversy,” in Philip Gleason, ed., *Catholicism in America* (New York, 1970), 68–78; Gleason, *Conservative Reformers: German-American Catholics and the Social Order*, 14–68, 145–58; and Glazer, *American Judaism*, 22–42.

in the politics of foreign policy. By the 1920s the resistance of the separate German religious communities helped to abort plans for a German-American bloc in national elections.⁴⁷ The unity which Bismarck imposed on a religiously plural Germany turned out in the American setting to be neither socially nor politically feasible.

A similar sharpening of the religious boundaries of ethnic association took place among Magyar immigrants, whom other Americans often labeled, inaccurately, "Hungarians." After 1867 the Kingdom of Hungary conferred special privileges on minorities that accepted Magyarization in language and political loyalty, as did most of the Jews in the kingdom and scattered groups of Greek Catholic Rusins and Lutheran Germans. After 1903 the Hungarian prime minister extended this policy to Magyars and Rusins in the United States, secretly subsidizing congregations and newspapers whose leaders agreed to resist Americanization and to help keep their followers loyal to both the government and the religious organizations of their homeland. But the subsidies merely helped the Magyars in the United States do more conveniently what immigrants from other nations did on their own: develop increasingly separate ethnic communities—Catholic, Reformed, Lutheran, and Jewish—each sustained by a broad array of congregations, mutual benefit societies, and cultural associations. Until the 1930s, "Hungarian" synagogues stood aloof from both Reform and Orthodox rabbinical organizations; eventually, however, they did what some Magyar Catholics perceived at the outset was inevitable and became an ethnic subcommunity within a major religious tradition. Magyar Lutherans, most of whom joined the Missouri Synod, and the majority of the Reformed congregations, which accepted membership (and subsidies) from either the Presbyterian or Evangelical and Reformed denominations, followed a similar course.⁴⁸

The Rusins, often called Ukrainians or Ruthenians, illustrate particularly well the role of religion in setting new ethnic boundaries. For decades the First Greek Catholic Union and its affiliated organizations defined the ethnic identity of persons of that faith so completely that Slovaks who had been Greek Catholics in Europe became Ruthenians in the United States. During the 1890s wholesale conversions of Greek Catholic Rusins to Orthodoxy in Minnesota, Pennsylvania, and Ohio produced virtually all the "Russians" who lived outside California before 1920, a situation only slightly altered by the arrival after both world wars of refugees more accurately labeled "Rus-

⁴⁷ Frederick Luebke, "Leadership among German Americans between the World Wars," paper read at the Schouler Lectures and Symposium, Johns Hopkins University, February, 1976, pp. 6-8, 23-25.

⁴⁸ Benkart, "Religion, Family, and Community among Hungarians Migrating to American Cities, 1880-1930," 59-101; *The Jewish Review and Observer* (Cleveland), October 17 and 31, 1913, p. 5, p. 3; Louis A. Kalassy, "The Educational and Religious History of the Hungarian Reformed Church in the United States" (Ph.D. dissertation, University of Pittsburgh, 1939), 23-26, 65-72, 132; Dyrud, "The Rusin Question in Europe and in America, 1890-World War I," 227-74; and records in the office of the Hungarian Lutheran Church, Cleveland. I differ with Herberg about the outcome of this process; see his *Protestant-Catholic-Jew: An Essay in Religious Sociology*, 72-98, 254-81. It did not constitute a prostitution of religious values to Americanism but in some ways was a fulfillment of their character and promise. Also see Harold J. Abramson, "The Religio-Ethnic Factor and the American Experience: Another Look at the Three-Generations Hypothesis," *Ethnicity*, 2 (1975): 163-77.

sian.”⁴⁹ One of the few exceptions to the rule that faith defined ethnic boundaries in America was a third contingent of Rusins—those of both Greek Catholic and Orthodox affiliation—whose political aspirations were so powerful in both the Old World and the New that they, their prelates, and their priests were swept into the Ukrainian national movement.⁵⁰

The consolidation of New World ethnicity and Old World nationalism in Boston’s Albanian Orthodox community is almost a caricature of the prevailing pattern. Albania was originally Catholic in the north and Orthodox—under Greek bishops—in the south, but central Albania became Muslim as a result of the Ottoman conquest. In the 1890s nationalists in the south began pressing the Greek hierarchs for priests and a liturgy in their own language and for the early establishment of an independent (in Orthodox terminology, “autocephalous”) church. Albanians living outside the country in Bucharest, London, Cairo, and particularly America soon accomplished what the ecclesiastical authorities refused to allow in the home country. In 1903 Fan S. Noli, a native of Thrace who had lived for a time in Athens, moved to Egypt, where he met Orthodox nationalists. Three years later they sent him to Buffalo, New York to assist in the development of an Albanian mutual benefit society. The next year a Greek priest in Hudson, Massachusetts refused to allow a funeral liturgy for a young Albanian nationalist on the grounds that all such persons were automatically excommunicated. The members of the Boston community were outraged, and they persuaded Noli, in order to have him become their pastor, to accept ordination at the hands of the Russian archbishop in New York. Consecrated archbishop, Noli made the Albanian Orthodox Church, formally declared autocephalous in 1919, the center of the Albanian national movement. He translated and published for worldwide distribution an Albanian liturgy, printed textbooks in the native language for use in homeland and American schools, and influenced Catholic and Muslim leaders in the New World to use the pulpit as a forum to promote both Albanian nationalism and the ethnic advancement of their countrymen who had settled in America. When the end of the First World War brought independence to Albania, Noli returned to a land that had never been his home to become one of its first prime ministers.⁵¹

Much additional evidence of the overriding influence of religion upon identity-formation among other ethnic groups does not need recounting here. I have written elsewhere of the duality of communities, one Christian and the

⁴⁹ Andrew J. Shipman, “Greek Catholics in America,” *Catholic Encyclopedia*, 15 vols. (New York, 1907–12), 6: 745–49, and “Our Russian Catholics: The Greek Ruthenian Church in America,” *The [Russian Orthodox] Messenger*, 42 (November, 1912): 664–67; Stephen E. Gulovich, *Windows Westward: Rome, Russia, Reunion* (New York, 1947), 124–35; Walter C. Warzewski, “Religion and National Consciousness in the History of the Rusins of Carpatho-Ruthenia and the Byzantine Rite Pittsburgh Exarchate” (Ph.D. dissertation, University of Pittsburgh, 1964), 118–39; Alex Simirenko, *Pilgrims, Colonists, and Frontiersmen: An Ethnic Community in Transition* (Minneapolis, 1964), 37–54; Dimitry Gregorieff, “The Historical Background of Orthodoxy in America,” *St. Vladimir’s Seminary Quarterly*, 5 (1961): 9–12, 41; and Dyrud, “The Rusin Question in Eastern Europe and in America, 1890–World War I,” 136–69, 211–12.

⁵⁰ Ukrainian National Association, *Jubilee Book: In Commemoration of the Fortieth Anniversary* (Jersey City, N.J., 1933), 194–98, *passim*.

⁵¹ Skendi, *Albanian National Awakening, 1878–1912*, 6–13, 158–63, 296–303.

other anticlerical or socialist, that emerged around 1900 among Catholic Slovenes and Lutheran Finns. A brief effort to unite all South Slavs in America on the basis of their common descent—as nationalists dreamed of doing in the old country—ran afoul of the Slovenes' belief that they were culturally superior to their fellow Catholic Croatians and the Orthodoxy of the Serbs. Meanwhile, Montenegrin immigrants and those Orthodox Macedonians whose language was Serbo-Croatian became Serbs in America. Slovaks of Catholic, Lutheran, Greek Catholic, and Presbyterian backgrounds managed to unite for a decade or so after 1890 in the National Slovak Society, whose strongly political ideology was conditioned by long resistance to Magyarization in the old country; but congregations and mutual benefit societies soon formed separate national religious brotherhoods that proved to be the decisive catalysts of ethnicity and, eventually, the major avenues of accommodation to a wider American identity.⁵²

The very recent emergence of working unity among Catholic Hispanic-Americans illustrates the continuing role of religious affiliation in the evolution of the boundaries of ethnicity. To those Chicanos whose roots lay deep in the soil of the Rio Grande and lower Colorado valleys and in the southern California coastlands, the Anglos were the immigrants—but immigrants who held all the economic and political aces. The twentieth-century movements for social justice among Catholics, however, eventually produced in the American Southwest a band of nuns, teaching brothers, priests, and auxiliary bishops committed to promoting the welfare of Chicanos and of recent Mexican immigrants, whether in that region or in the shanty-towns that housed migratory agricultural workers from the Gulf states to Canada. This development roughly coincided with the emergence in eastern cities of a community of economic and political interests among groups of Catholics from Puerto Rico, Cuba, and other Latin American countries.⁵³ During the 1970s liberation theology and the quest of Cesar Chavez and others for political clout in the large states of California, Texas, Florida, and New York brought Chicanos and urban Hispanics together. They built their unity, however, within the scaffolding of relationships, beliefs, and social aspirations that they shared as Catholics. In October 1976 a Hispanic-American caucus exercised immense, though unpublicized, influence at the United States Bishops' Conference on Liberty and Justice for All, the first representative assembly of American Catholic clergy and laity held in the twentieth century. On the heels of that achievement, the Hispanic-American members of the United States Congress announced success in the long-frustrated effort to form a congressional caucus. If, as I believe, these developments affirm the mobilization of an enlarged

⁵² Smith, "Religious Denominations as Ethnic Communities: A Regional Case Study," 210–24; and Mark Stolarik, "Building Slovak Communities in America," paper read at the Hopkins-Harwichport Seminar in American Religious History, August 1974.

⁵³ Joseph Fitzpatrick, *Puerto Rican Americans* (Englewood Cliffs, N.J., 1971); Leo Grebler, J. W. Moore, and R. C. Guyman, *The Mexican-American People: The Nation's Second Largest Minority* (New York, 1970), 453–57, 463–68; and Manuel P. Servin, ed., *The Mexican-Americans: An Awakening Minority* (Beverly Hills, Calif., 1970), 176–78, 188–92, 232–33.

and powerful ethnic entity, its political as well as its economic significance reflects the successful linking of ethnic interests to religious idealism in broad strategies that are as much Catholic as Hispanic.⁵⁴

THE CUSTOMS AND BELIEFS of particular varieties of faith and the traditions of loyalty to them seem, then, to have been the decisive determinants of ethnic affiliation in America. The availability of religious structures whose rituals were rooted in the past but whose doctrines sustained expansive hopes for the future does not, however, fully explain the changing relationship of faith to ethnicity in the United States. A second important alteration in that relationship was the intensification of the psychic basis of religious commitment that the acts of uprooting, migration, and repeated resettlement produced in the minds of new Americans. The most important of the several enduring contributions of Oscar Handlin in *The Uprooted* is his evocation of the anxieties, both personal and social, that resulted from forsaking an old home and searching for a new community. An intense interest in the religious meaning of their break with the past lay behind the preoccupation of both clergy and lay emigrants with religious organizations; and this interest stemmed from formidable psychic challenges.⁵⁵

The individual's sense of responsibility for the decision to migrate was primary here. Loneliness, the romanticizing of memories, the guilt for imagined desertion of parents and other relatives, and the search for community and identity in a world of strangers all began the moment the nearest range of hills shut out the view of the emigrant's native valley. Longing for a past that could not be recovered intensified the emotional satisfaction of daring to hope for a better future. Separation from both personal and physical associations of one's childhood community drew emotional strings taut. Friendships, however, were often fleeting; and the lonely vigils—when sickness, unemployment, or personal rejection set individuals apart—produced deep crises of the spirit. At such moments, the concrete symbols of order or hope that the village church and priest and the annual round of religious observances had once provided seemed far away; yet the mysteries of individual existence as well as the confusing agonies of anomie cried out for religious explanation.⁵⁶ For this

⁵⁴ This development is contrary to what Andrew Greeley expected only five years ago; see his *Ethnicity in the United States: A Preliminary Reconnaissance*, 295. My account is based on interviews with officers of the Mexican American Cultural Center in San Antonio. As a consultant to the Bishops' Conference on Liberty and Justice for All, I witnessed the immense influence exercised by the Hispanic-American caucus. Also see Jacques E. Levy, *Cesar Chavez: Autobiography of La Causa* (New York, 1975), 453–62; and Francis P. Firenza, "Latin American Liberation Theology," *Interpretation*, 28 (1974): 441–57. For eschatological, ecumenical, and biblical perspectives, see Juan Luis Segundo, S. J., *Liberation of Theology*, trans. John Drury (New York, 1976), 138–51, 228–37.

⁵⁵ Handlin, *The Uprooted: The Epic Story of the Great Migrations that Made the American People* (Boston, 1951). Harold Isaacs has seen the psychic intensity but not the theological implications of uprooting and migration; "Basic Group Identity: Idols of the Tribe," 34, *passim*. But such theological implications gave ideological and emotional stamina to ethnoreligious groups; see Timothy L. Smith, "Refugee Orthodox Congregations in Western Europe, 1945–1948," *Church History*, 38 (1969): 320–26; and Stanford E. Marovitz, "The Lonely New Americans of Abraham Cahan," *American Quarterly*, 20 (1968): 197. For comparison, see John A. Jackson, *The Irish in Britain* (London, 1963), xi–xii, 137, 142–44.

⁵⁶ For my brief discussion of this matter, see "Congregation, State, and Denomination: The Forming of

reason, I shall argue, migration was often a theologizing experience—just as it had been when Abraham left the land of his fathers, when the people of the Exodus followed Moses into the wilderness, and when Jeremiah urged the exiles who wept by the rivers of Babylon to make the God of their past the hope of their future.

Preoccupation with the ethical dimension of faith was one outcome of such uprooting. Once in America, immigrants uniformly felt that learning new patterns of correct behavior was crucial to their sense of well-being. Everything was new: the shape and detail of houses, stairways, windows, and stoves; the whirl of engines, trolleys, furnaces, and machines; the language, facial expressions, dress, table manners, and forms of both public and private courtesy; and, most important of all, freedom from the moral constraints that village culture had imposed in matters monetary, recreational, occupational, alcoholic, educational, and sexual. Each immigrant had to determine how to act in these new circumstances by reference not simply to a dominant “host” culture but to a dozen competing subcultures, all of which were in the process of adjustment to the materialism and the pragmatism that stemmed from the rush of both newcomers and oldtimers to get ahead.⁵⁷ This complex challenge to choose among competing patterns of behavior affected immigrants in all periods of American history; and they persisted in dealing with it on religious terms. At the turn of the twentieth century, Father Paul Tymkevitch declared that the greatest need of young Rusin Greek Catholics was to acquire “habits”—patterns of behavior ratified by both conscience and example and imprinted by repetition, patterns that would make each person his or her own monitor.⁵⁸

But which cultural home should a young man choose? The tradition-oriented group that had preceded him here from the old country and presented itself as guardian of a past he sensed must disappear? The value system of the Americanizing culture-brokers living on the fringes of his own community? The culture of what he perceived to be a “successful” immigrant group that

an American Religious Structure,” 156–60. For examples from several settings of such sentiments, see Francis Asbury, *Journal and Letters*, ed. J. Manning Potts et al., 1 (Nashville, 1958): 1, 8, 39–40, 72, 77, 123–25; Donald F. Durnbaugh, ed., *The Brethren in Colonial America: A Source Book on the Transplantation and Development of the Church of the Brethren in the Eighteenth Century* (Elgin, Ill., 1967), 229–31; Charlotte J. Erickson, *Invisible Immigrants: The Adaptation of English and Scottish Emigrants in Nineteenth-Century America* (Coral Gables, Fla., 1972), 73–74, 112, 127–28, 182; Alexander S. Salley, Jr., ed., *Narratives of Early Carolina, 1650–1708* (New York, 1911), 175–76; James W. C. Pennington, *The Fugitive Blacksmith; or, Events in the History of James W. C. Pennington, Pastor of a Presbyterian Church, New York, Formerly a Slave . . .* (London, 1850), 67, 77–78; and Levy, *Chavez*, 23–27, 35–39. Also see Herbert G. Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976), 265–67.

⁵⁷ For my development of this point, see “A General Theory of Inter-Ethnic Relations among Peoples Migrating from the Old World to the New,” paper read at the Hopkins-Harwichport Seminar in American Religious History, August 1974. Milton Gordon, in contrast with Victor Greene, has remained preoccupied with majority-minority issues; see Gordon, “Toward a General Theory of Racial and Group Relations,” 101–05; and Greene, *For God and Country: The Rise of Polish and Lithuanian Consciousness in America, 1860–1910*, chaps. 4, 5, 8.

⁵⁸ For the Tymkevitch story, see Emily Greene Balch, “A Shepherd of Immigrants,” *Charities*, 13 (1904): 193–94. Also see Gollin, *Moravians in Two Worlds*, 16; and Robert Mirak, “On New Soil: The Armenian Orthodox and Armenian Protestant Churches in the New World to 1915,” in Miller and Marzik, *Immigrants and Religion in Urban America*, 139–40, 150–54, esp. 153.

settled here earlier than his own? One or another of the “native” American subcultures shared by persons of his religious faith? Or the secular and hence nonethnic culture of the wider “urban community,” which he identified with mass communications, politics, popular entertainment, and a soulless economic order? When one personal crisis or another prompted fears of the dissolution of the person the young immigrant thought he had been, ordinary questions of behavior led into more profound ethical ones, setting the terms by which the religion of his forbears had to respond. Occasionally, the response was a radically perfectionist one.⁵⁹

What Marcus Lee Hansen has called “immigrant Puritanism” owed virtually nothing to colonial New England. It was, rather, a predictable reaction to the ethical or behavioral disorientation that affected most immigrants, whatever the place or the century of their arrival. The surprising attraction of the nineteenth-century total abstinence movement to Irish, German, and Slovenian Catholics, as well as to Finnish Lutherans in Minnesota and Massachusetts, illustrates the force of that reaction.⁶⁰ Europeans sometimes complain that American religion is too much concerned with ethical behavior and too little with theological reflection. But to a nation composed of so many migrating peoples, action—right action—was the name of the game. The immigrant’s religion needed both rule and the reformation of rules, both the law and the prophets.⁶¹

Once conceptions of identity and proper behavior had been wrenched loose from the past, the theological interest of new settlers moved naturally to a deep fascination with the future. From its colonial beginnings, the migration of bonded groups or the formation of such groups in the new land made the biblical imagery of the Exodus seem a metaphor for the American experience, not only for English Puritans and Russian Jews, but for Christian villagers of Catholic, Protestant, and Orthodox persuasions from all parts of Europe. If the last great wave did not find urban America to be the promised land, the

⁵⁹ Insofar as religious perfectionism has been pervasive in American religious life, its major social impulse lies here. For a discussion of this subject in a somewhat narrower framework, see my *Revivalism and Social Reform in Mid-Nineteenth-Century America* (Nashville, 1957), 103–47. For immigrant perfectionist responses, see Arlow W. Anderson, *The Salt of the Earth: A History of Norwegian-Danish Methodism in America* (Nashville, 1962), 12–17, 28–32; and Solomon Schechter, *Seminary Addresses & Other Papers* (New York, 1915), 42–43, 196–97. For Catholic examples, see Frederic W. Faber, *Growth in Holiness; or, The Progress of the Spiritual Life* (Baltimore, 1855), 1, 15, 34, 130–47; Dolan, *Catholic Revivalism in the United States, 1830–1900*, 178–79, 182; and Coleman and LaBud, *Monsignor Joseph F. Buh, Slovenian Missionary in America, 1864–1922*, 130–31.

⁶⁰ Marcus Lee Hansen, *The Immigrant in American History* (Cambridge, Mass., 1949), 97–128; Coleman and LaBud, *Monsignor Joseph F. Buh, Slovenian Missionary in America, 1864–1922*, 166–99, 226–27; James E. Brady, “Father George Zurcher: Prohibitionist Priest,” *Catholic Historical Review*, 52 (1976): 426–29; Dolan, *Catholic Revivalism in the United States, 1830–1900*, chap. 6; “Temperance and Temperance Movements,” *Catholic Encyclopedia*, 14: 482–93; and Smith, “Religious Denominations as Ethnic Communities: A Regional Case Study,” 210–12. Also see the passage on “Moralism in American Jewish Life” in Joseph L. Blau, *Judaism in America: From Curiosity to Third Faith* (Chicago, 1975), 69–72; and Martin E. Marty, *A Nation of Behavers* (Chicago, 1976), 33–51, 158–79.

⁶¹ For a discussion of the moral preachments of the *maggid* in late nineteenth-century London Jewry, see Lloyd P. Gartner, *The Jewish Immigrant in England, 1870–1914* (London, 1960), 189–91; and, for perceptive comments on the relationship of peasant society to an emerging “ecumenical” moral order geared to the “technical order” of urban civilization, see Robert Redfield, *Human Nature and the Study of Society: The Papers of Robert Redfield*, ed. Margaret P. Redfield, 1 (Chicago, 1962): 282–94.

vision of what William Ellery Channing called, in millennial and Protestant terms, "a Better Day" remained pervasive among them. Charismatic leaders in all ethnic groups were messianic and anticipated something like a New Jerusalem. Linking the American future with the Kingdom of God was not, therefore, an exclusively Yankee obsession, nor the Social Gospel a Protestant preserve. Jews of both Reform and Orthodox faith, radical Irish as well as Chicano Catholics, and Mormon converts from Europe (whose trek to the Great Basin Kingdom seemed at first a flight away from civilization rather than a pilgrimage toward a better one) have also been people of the dream.⁶²

Out of this pervasive social idealism emerged a conviction among both Jewish and Christian groups that theirs were pilgrim peoples, who had by their own choice responded to a divine call and made a covenant to walk with God. Covenant theology, which Perry Miller has demonstrated was central to the Puritan "errand in the wilderness," turns up at least occasionally in the faith of almost every American ethnic group.⁶³ We are not surprised, of course, to find Dutch Reformed, Scottish and Scotch-Irish Presbyterians, French Huguenots, and Reformed Magyars voicing this theme; their heritage in the Genevan Reformation, whether filtered through the Westminster Assembly of Divines or not, is obvious. But when Mennonites and Pietist Moravians sing about it in colonial Pennsylvania and pioneer Baptists, German Lutherans, and Scandinavian free-churchmen in the Ohio Valley pick up the refrain, we must stop to listen. We hear echoes of the same conviction in the appeals of nineteenth-century Irish, German, and French Catholic missionaries to the Midwest and, later, in the pastoral addresses of Eastern Orthodox bishops. The doctrine of a people in covenant with God attracted individuals who in the American maze way bonded themselves in faith with persons whom they would have regarded as strangers in their homelands.⁶⁴ Only in the last few decades have students of the ancient Near East discovered evidence that belonging in ancient Israel may have rested less on descent from

⁶² Ernest Lee Tuveson, *Redeemer Nation: The Idea of America's Millennial Role* (Chicago, 1968); William Ellery Channing, *The Perfect Life* (1873; reprint ed., Boston, 1901), 116–19, 194–96, *passim*; and Coleman and LaBud, *Monsignor Joseph F. Buh, Slovenian Missionary in America, 1864–1922*, 186. Also see Max Vorspan and Lloyd P. Gartner, *History of the Jews of Los Angeles* (San Marino, Calif., 1970), 157; Francis, *In Search of Utopia: The Mennonites of Manitoba*, 5, 35–39, 81–86; William Mulder, *Homeward to Zion: The Mormon Migration from Scandinavia* (Minneapolis, 1957), 137–45, 149, 154–55, 163, 178; and Forster, *Zion on the Mississippi: The Settlement of the Saxon Lutherans in Missouri, 1839–1841*, 87, 134–36, 151–52.

⁶³ Miller, "The Marrow of Puritan Divinity," in *Errand into the Wilderness* (Cambridge, Mass., 1956), 48–98. But George H. Williams has suggested that immigrant congregations were not preoccupied with the future, but looked either backward or upward; Williams, "The Wilderness and Paradise in the History of the Church," *Church History*, 28 (1959): 6–7.

⁶⁴ Robert Friedmann, "The Doctrine of the Two Worlds," in Guy F. Hershberger, ed., *The Recovery of the Anabaptist Vision* (Scottsdale, Pa., 1962), 110–13, 116–17; Gollin, *Moravians in Two Worlds*, 13–19; John F. Cody, *The Origin and Development of the Missionary Baptist Church in Indiana* (Franklin, Ind., 1942), 30, *passim*; Forster, *Zion on the Mississippi: The Settlement of the Saxon Lutherans in Missouri, 1839–1841*, 87, 134–36, 151–52; Armas K. Holmio, "The Beginnings of Finnish Church Life in America," in Ralph J. Kalkanen, ed., *The Faith of the Finns: Historical Perspectives on the Finnish Church in America* (East Lansing, Mich., 1972), 127–28; Simirenko, *Introduction to Pilgrims, Colonists, Frontiersmen: An Ethnic Community in Transition*; Henry Zwaansstra, *Reformed Thought and Experience in a New World: A Study of the Christian Reformed Church and Its American Environment, 1890–1918* (The Hague, 1973), 91–93; and George Dolak, *A History of the Slovak Evangelical Lutheran Church in the United States of America, 1902–1927* (St. Louis, 1955), "Epilog," 163. Also see Max Weber, "Ethnic Groups," 305–08.

Jacob than upon an act of commitment—that is, on the willingness of a company of former slaves from Egypt and of persons who became their confederates in the land of Canaan to own the Covenant of Righteousness and call themselves Jahweh's people.⁶⁵

Accompanying this fascination with a covenanted future was an extensive personalizing of religious faith—a process frequently confused with making it private or individualistic. True, the experience of uprooting and resettlement was a remarkably solitary one, and the traumatic aspects of it affected each person individually. The result, however, was not to make individual experience the measure of faith but to enlarge the sense of personal involvement in one's religious community and in its systems of belief and prescriptions for behavior. Immigrant congregations served diverse family, group, and individual interests. They were not transplants of traditional institutions but communities of commitment and, therefore, arenas of change.⁶⁶ Often founded by lay persons and always dependent on voluntary support, their structures, leadership, and liturgy had to be shaped to meet pressing human needs.⁶⁷ The same was true of the regional and national ethnic denominations or sub-communities which emerged in America. They had to justify themselves by nurturing those morally transforming experiences that the whole membership perceived to be “saving.”⁶⁸

Pastors, rabbis, and lay officers responded to this challenge to make religion more personal by reinterpreting scriptures and creeds to allow ancient observances to serve new purposes. The nineteenth-century Protestant custom of a weekly “social meeting” for prayer, testimony, and mutual encouragement was not the creation of native-born Baptist or Methodist enthusiasts. Nor was it a popularization of John Wesley's “class meeting.” Drawing upon the German tradition of Pietist conventicles, an immigrant pastor, Philip William

⁶⁵ For a summary of the scholarship of the preceding twenty years, see Edward F. Campbell, “Moses and the Foundation of Israel,” *Interpretation*, 29 (1975): 141–54.

⁶⁶ William Warren Sweet taught a generation of historians that revivalism made religion more individualistic; Sweet, *Revivalism in America* (New York, 1945), xi–xiv, *passim*. Also see William G. McLoughlin, ed., Introduction to *The American Evangelicals, 1800–1900* (New York, 1968); Marty, *A Nation of Behavers*, 15, 34. But see Kukkonen, “The Influence of the Revival Movements of Finland on the Finnish Lutheran Churches in America,” 132. W. Clark Roof has argued that substantive commitment, not organizational affiliation, should be the central conceptual concern in analyzing religious congregations; Roof, “The Local-Cosmopolite Orientation and Traditional Religious Commitment,” *Sociological Analysis*, 33 (1972): 1–3.

⁶⁷ This is a key point in my “Congregation, State, and Denomination: The Forming of the American Religious Structure.” Also see Dolan, *The Immigrant Church: New York's Irish and German Catholics, 1815–1865*, 53–84; Briggs, “Church Building in America: Divergent and Convergent Interests of Priests and Lay People in Italian-American Communities”; Daniel S. Buczek, *Immigrant Pastor: The Life of the Right Reverend Monsignor Lucyan Bójnowski of New Britain, Connecticut* (Waterbury, Conn., 1974), 13–16, 18, 39–52, 63; and Maxwell Whiteman, “Philadelphia's Jewish Neighborhoods,” in Allen F. Davis and Mark F. Haller, eds., *The Peoples of Philadelphia: A History of Ethnic Groups and Lower-Class Life, 1790–1940* (Philadelphia, 1973), 232–37.

⁶⁸ Conzen, *Immigrant Milwaukee, 1836–1860*, 159–68; Vorspan and Gartner, *History of the Jews of Los Angeles*, 55–58, 154–64; Dolan, *The Immigrant Church: New York's Irish and German Catholics, 1815–1865*, 64–66, 75–77; and Larkin, “The Devotional Revolution in Ireland, 1850–1875,” 649–52. Also see Gotthardt D. Bernheim, *History of the German Settlements and of the Lutheran Church in North and South Carolina . . .* (Philadelphia, 1872), 205–24, *passim*.

Otterbein, fashioned the new institution on the eve of the Revolutionary War in order to cultivate "inward Christianity" among his parishioners. Two examples of shifting emphases in Jewish rituals stand out. The steady elaboration of the ceremony of bar mitzvah reflected communal anxieties about growing up in a pluralistic society that did not recognize the norms of the subculture. And the intensified observance of kaddish (prayers for the dead) testified less to the strength of tradition than to the deepening sense of the significance of individual life in a world of relentless change.⁶⁹ The many forms of religious revivalism, I have argued elsewhere, were not "individualistic" in the usual sense that term suggests; though they made faith a profoundly personal experience, their aim and outcome was to bind individuals to new communities of belief and action.⁷⁰

Notions of pilgrimage and expectations of personal and cultural change magnified concern for a basis of moral and religious authority that could provide a sense of permanence to those adapting themselves to shifting social realities. Catholic and Jewish concepts of the authority of tradition, which in theory made the interpretation of biblical truth adaptive, gave place steadily to reliance upon the Bible itself. The result, however, was not to throttle but to enhance the freedom of immigrant pastors and rabbis to adjust faith and practice to new situations. True, the doctrine of papal infallibility—proclaimed, as chance would have it, in the period of greatest Catholic migration—attempted to freeze out innovative appeals to the Scriptures by asserting the ultimate authority of those traditions ratified by Rome.⁷¹ During the same era Protestant fundamentalists, in a doubly ironic development, employed a largely new conception of the Bible's verbal inerrancy to promote innovations in millenarian doctrines as well as to protect other, older doctrines from the rise of scientism and religious modernism.⁷² Immigrant congregations, however, were in the vortex of change and found custom and tradition—whether rabbinical, denominational, or pontifical—insufficient to chart their path through a world that seemed increasingly like Alice's moral Wonderland.

⁶⁹ For an incisive summary of the new—"modern"—meanings Czech peasants attached to old rituals, ceremonies, and festivals, see Barton, "Religion and Cultural Change in Czech Immigrant Communities," 10–11, 15. Also see Miller, "Irish Catholicism and the Great Famine," 87–93; and O'Dea, "Stability and Change and the Dual Role of Religion," 161–65, 171–72. For the Otterbein story, see Tanis, "Reformed Pietism in Colonial America," 70–72. My interpretation of Jewish rituals is based on a conversation with Lloyd P. Gartner in November 1976. For Irish resistance to the efforts of their priests to curb extensive wakes, see Dolan, *The Immigrant Church: New York's Irish and German Catholics, 1815–1865*, 60–62, 92.

⁷⁰ Smith, "Congregation, State, and Denomination: The Forming of the American Religious Structure," 174–75, and *Revivalism and Social Reform in Mid-Nineteenth-Century America*, 80–88. Also see Donald Mathews, "The Second Great Awakening as an Organizing Movement," *American Quarterly*, 21 (1969): 23–43; and Rhys Isaac, "Preachers and Patriots: Popular Culture and the Revolution in Virginia," in Alfred E. Young, ed., *The American Revolution: Explorations in the History of American Radicalism* (DeKalb, Ill., 1976), 138–40.

⁷¹ For a wise review of the "progressive" view of scriptural authority which papal action suppressed, see James T. Burtchaell, *Catholic Theories of Biblical Inspiration since 1810: A Review and Critique* (London, 1969), 282–305.

⁷² Ernest R. Sandeen, *The Roots of Fundamentalism: British and American Millenarianism, 1800–1930* (Chicago, 1970), 103–31.

As in the cases of migrating Quakers and Mennonites in colonial Pennsylvania, growing attachment of nineteenth-century ethnic denominations to the authority of the Bible stemmed from expanding perceptions of the relevance of its central principles to new situations.⁷³ Leaders of the Christian Reformed Church, composed of Dutch Calvinists who emigrated to the upper Midwest after the American Civil War, explicitly disavowed both Old World social customs and their own formal commitment to a literal interpretation of Scripture when they supported the prohibition movement. The broader biblical principle of love for one's neighbor and of responsibility to bear his burdens, they said, was in this case binding. For two decades Swedish Mission Covenant congregations in America were able to cooperate with the Congregationalists at Chicago Theological Seminary because Paul P. Waldenström, their spiritual leader in Scandinavia, had taught them to use the Bible to cast off Lutheran scholasticism. His progressive use of scriptural authority—which exactly paralleled that of America's mid-nineteenth-century social reformers—held back his emigrant coreligionists from fundamentalism, even though they were committed to preserving the “fundamental” doctrines of Christian faith.⁷⁴

Major spokesmen for Judaism also displayed growing attachment to scriptural as against traditional authority. Isaac M. Wise, in a series of essays and addresses in support of Reform Judaism, appealed to the biblical prophets and to modern rational judgment in declaring that the Ten Commandments were the essence of the Torah. The “Law of Moses,” comprising the body of the Pentateuch, “reduces to practice the fundamental concept of the Decalogue,” he said, “and expounds and expands its doctrines”; but the detailed provisions of Mosaic law were binding only upon Jews residing in the Land of Promise. The Ten Commandments are, however, “eternal law and doctrine,” Wise declared. Progressive and law-abiding Jews must decide in the light of both conscience and reason which of their traditional rules and customs would fulfill those principles in the lands of their wandering and lead them to “salvation by righteousness.” Solomon Schechter declared at the dedication

⁷³ C. Norman Kraus, “American Mennonites and the Bible, 1750–1950,” *Mennonite Quarterly Review*, 41 (1971): 320–23. Forster has recounted the crisis of discredited allegiances amidst which the Lutheran Missouri Synod's biblicism emerged; *Zion on the Mississippi: The Settlement of the Saxon Lutherans in Missouri, 1839–1841*, 75, 82, 173, 279, 464–66, 520. But see Milton L. Rudnick, *Fundamentalism & the Missouri Synod: A Historical Study of Their Interaction and Mutual Influence* (St. Louis, 1966), 68, 75–84. Armas Holmio and Walter Kukkonen have stressed the socially adaptive aspects of revivalist biblicism in Finland, but they have shown the potential for a rigid defense of the objective “word” that developed in one of the three branches of Finnish Lutherans in the United States; Holmio, “The Beginnings of Finnish Church Life in America,” 112–17; and Kukkonen, “The Influence of the Revival Movements of Finland on the Finnish Lutheran Churches in America,” 122–27.

⁷⁴ Zwaanstra, *Reformed Thought and Experience in a New World: A Study of the Christian Reformed Church and Its American Environment, 1890–1918*, esp. 220–23; and, for other evidences of a progressive adaptation of religious views, see *ibid.*, 49–58, 239–50; Frederick A. Hale, “Trans-Atlantic Conservative Protestantism in the Evangelical Free and Mission Covenant Traditions” (Ph.D. dissertation, Johns Hopkins University, 1977), 143–53, 294–97; and Stephenson, *Religious Aspects of Swedish Immigration*, 106–09. For progressive implications of Scandinavian free-church migration, see Anderson, *Norwegian-Americans*, 95–108; and Blegen, *Norwegian Migration to America: The American Transition*, 131–52. For German Catholic progressivism, see Gleason, *Conservative Reformers: German-American Catholics and the Social Order*, 116–44. Also see Forster, *Zion on the Mississippi: The Settlement of Saxon Lutherans in Missouri, 1839–1841*, 53.

of the Jewish Theological Seminary of America in 1903 that "this country is, as everybody knows, a creation of the Bible, particularly of the Old Testament." And the Bible is "still holding its own," he continued, despite the rising influence of higher criticism, which Schechter called "Higher Anti-Semitism."⁷⁵

Uprooted persons seeking a new community needed both a principle of authority and a dynamic and essentially "progressive" use of it. Their way of relying on Scriptures placed them in what had become the mainstream of American evangelical thought.⁷⁶ Even in the colonial period America was not settled by persons with radical religious views; either they were progressives or became so here. They were restrained from radicalism by the value they placed on their past and from reaction by their faith in a better future. In *Herzog* Saul Bellow expressed precisely one dimension of this American mood: "Personal responsibility for history, a trait of Western culture, rooted in the Testaments, Old and New, the idea of the continual improvement of human life on earth. What else explained Herzog's ridiculous intensity?"⁷⁷

In such ways the acts of uprooting, migration, resettlement, and community-building became for the participants a theologizing experience, not the secularizing process that some historians have pictured. If George Gallup's pollsters are even remotely accurate in their recent finding that Americans are second only to citizens of India in the pervasiveness and intensity of their religious beliefs and practices, the myths of wholesale secularization are no longer tenable.⁷⁸ Migration into modernity may have finished off the hilltop gods of village, tribe, and monastic retreat; but it neither dethroned in Jewish minds the God of Israel nor destroyed in Christian understanding the lordship of Jesus over history. The folk theology and religious piety that flourished in immigrant churches from the beginnings of American settlement were not merely traditional but progressive. Belief and devotion were powerful impulses to accommodation and innovation; and both helped legitimate the behavior, the perceptions, and the structures of association that sustained the processes of change.⁷⁹

⁷⁵ Isaac M. Wise, *Essays and Addresses* (1901; reprint ed., New York, 1969), 133-34, 151-52, 157, 159-63; and Schechter, *Seminary Addresses & Other Papers*, 36-37, 48-49. The ethical emphases in Wise's sermon on the character of Moses exactly parallel those of William Ellery Channing's sermon, preached forty years earlier, on "Jesus Christ, the Brother, Friend, and Saviour." For the text of the latter, see Channing, *The Perfect Life*, 238-40. For the importance of the Bible to Swedish Methodists, see Stephenson, *Religious Aspects of Swedish Immigration*, 118; and, for the centrality of biblical authority in Protestant social radicalism, see George D. Herron, *Social Meanings of Religious Experiences* (1896; reprint ed., New York, 1969), xiii-xxiv, *passim*.

⁷⁶ George M. Marsden, *The Evangelical Mind and the New School Presbyterian Experience* (New Haven, 1970), 99-100, 111-16, 142-50, 169-74; and Smith, *Revivalism and Social Reform in Mid-Nineteenth-Century America*, 101, 144, 147, 216-20. Jerry Wayne Brown has provided evidence that the most sophisticated and "progressive" biblical scholarship in New England emanated from evangelical Andover, though the structure of his book argues a Unitarian pre-eminence; see *The Rise of Biblical Criticism in America, 1800-1870: The New England Scholars* (Middletown, Conn., 1969), 46-63.

⁷⁷ Bellow, *Herzog* (1976), chap. 11.

⁷⁸ Peter C. Wagner, "How Christian Is America?" *Christianity Today*, 21 (1976): 12-16.

⁷⁹ See Seymour Martin Lipset, *The First New Nation: The United States in Historical and Comparative Perspective* (London, 1963), 140-41, 158-69. The religious impulse to innovation was, I think, one basis for the working-class political progressivism described by J. Joseph Huthmacher; see his "Urban Liberalism and the Age of Reform," *Mississippi Valley Historical Review*, 49 (1962): 234-39.

The role of Protestant evangelicalism among American blacks confirms that religion contributed in this progressive way both to the formation and to the ideology of ethnic groups. The situation of blacks differed dramatically from that of Europeans, Asians, and Hispanic-Americans, whose forebears came to this country by choice. Such memories as persisted of their African religious traditions were fragmented and suppressed as slaveowners denied them the freedom to choose their neighbors and companions and to follow their own ways and times of religious observance. Partly as a consequence of this imposed disorientation, the psychic and the organizational bases of the sense of peoplehood matured only in the nineteenth century, after generations of enslavement.⁸⁰ Emancipation rather than migration, therefore, was the watershed that separated their “new world” from the old. By the time of emancipation, however, their vanguard was explicitly committed to constructing a future out of materials borrowed from white Protestants, particularly from the evangelical Methodists and Baptists, who had secured their conversion and sanctioned, though sometimes reluctantly, their desire for separate congregations. Consequently, for decades Protestant blacks in America made their churches the center of their social life and of their efforts at ethnic—blacks called it “racial”—progress.⁸¹

But consider the ideas that black preachers, once free, made central to their exposition of biblical faith: the doctrine of God’s providential rule over history; close identification with the exodus of a liberated people who they understood were chosen for service, not sovereignty, and by grace, not merit; the conviction that internalized moral law, a covenant of righteousness, was the only sure basis for social order; and the biblical—especially the New Testament—affirmation of human worth and personal identity in this world and the next.⁸² Whether these elements in the theology of nineteenth-century blacks were in any part reconstitutions of ideas that had prevailed earlier in Islamic or other African religions is not, from the point of view of this essay, a crucial question. They were, in fact, aspects of the Hebrew and Christian faiths that most American immigrants also believed gave divine sanction and direction to their long pilgrimage toward a more just, happy, and humane tomorrow.

⁸⁰ Eugene Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 161–284. Herbert Gutman has pushed a fine point too far in positing a cultural system among slaves—or a racial element in their bi-culturation—that rested exclusively on the ties of family and kinship. He has, however, convincingly demonstrated that these ties were present and powerful. See Gutman, *The Black Family in Slavery and Freedom*, 260–64.

⁸¹ For their ambivalence, see Leon F. Litwack, “Free at Last,” in Hareven, *Anonymous Americans: Explorations in Nineteenth-Century Social History*, 142–51, 158–66. Also see E. Franklin Frazier, *The Negro in the United States* (rev. ed., New York, 1957), 338–53; Theodore Hershberger, “Free Blacks in Antebellum Philadelphia,” in Davis and Haller, *The Peoples of Philadelphia*, 120–21; James W. St. G. Walker, “The Establishment of a Free Black Community in Nova Scotia, 1783–1840,” in Martin L. Kilson and Robert I. Rotberg, eds., *The African Diaspora: Interpretative Essays* (Cambridge, Mass., 1976), 205–07, 214–19; and David M. Tucker, *Black Pastors and Leaders: Memphis, 1819–1972* (Memphis, 1975), 41–54, *passim*.

⁸² See my “Slavery and Theology: The Emergence of Black Christian Consciousness in Nineteenth-Century America,” *Church History*, 41 (1972): 497–512. For a sophisticated updating of the idea of the Exodus in black theology, see James Cone, “Biblical Revelation and Social Existence,” *Interpretation*, 28 (1974): 422–40.

MIGRATION AND RESETTLEMENT, then, altered the relationship between faith and ethnic identity by redefining the boundaries of peoplehood and by intensifying religious reflection and commitment. But it also breathed new life into messianic and millennialist hopes for the unity of all mankind and consequently prevented the cementing of the new patterns of belonging into a permanent American mosaic. In a new nation faced from its beginnings with the problems of unity and diversity, the revitalization of religious convictions accentuated the claim of both Judaism and Christianity to universality and renewed the impulse, largely suppressed among Jews since the first century of the Christian era, to recruit all human beings into a common circle of faith and fellowship.

The idea of a common humanity stands at the center of all major Western religions, and each of the ethnosectarian versions of Jewish and Christian faith in America affirms it. The countercurrents were formidable, to be sure, and on many occasions proved the stronger. But from the beginning—recall Count Zinzendorf's dream of establishing in colonial Pennsylvania an ecumenical "congregation of God in the Spirit"—the conviction that ethnic religiosity is not enough, that biblical faith is both incisive and inclusive and celebrates both particular and universal values has been an important support to the integrative pluralism John Higham has recently described in more secular terms. The ethnic springs of modern American religiosity have given the national culture not a backwater of static dogmas and rituals but a many-channeled stream of conviction that mankind must become one people.⁸³

A few illustrations, drawn from both dominant and marginal groups in each faith, must suffice. Reform rabbis in America revived the ecumenical view of monotheism, which in the Book of Genesis was associated with a migratory and pilgrim family, and linked it to reason, science, and progressive democracy. Isaac Wise unashamedly called their conception of the future identical to the Social Gospel's "Kingdom of God."⁸⁴ At an opposite pole, the most marginal Jewish sectarians, the *Hasidim*, revived mystical and messianic visions that, in their expectations of the future as well as their search for righteousness through spiritual devotion, likewise broke out of the constraints that rabbinical Judaism sought everywhere to maintain. Thus, in both Reform Judaism and Hasidism, universalism sprang not from the denial of themes that had long been integral to Jewish religious thought but from an enlarged emphasis upon them.⁸⁵

Originally, Christianity was a Jewish sect. Jesus' "good news" was to fulfill

⁸³ David J. O'Brien has stressed the ideal of universal human brotherhood, as opposed to class or national interests, in Dorothy Day's Catholic Worker movement; O'Brien, *American Catholics and Social Reform: The New Deal Years* (New York, 1968), 201–04, 221–27. On Zinzendorf, see Gollin, *Moravians in Two Worlds*, 18–19; and Jacob John Sessler, *Communal Pietism among Early American Moravians* (New York, 1933), 20–71.

⁸⁴ Wise, *Essays and Addresses*, 218–20.

⁸⁵ Gershom Scholem, *The Messianic Idea in Judaism, and Other Essays in Jewish Spirituality* (New York, 1971), 194–95, 200–01. For a sober re-evaluation of Martin Buber's interpretation of Hasidism, see *ibid.*, 238–48. Also see Scholem, *Major Trends in Jewish Mysticism*, 337–44; and Fackenheim, *God's Presence in History: Jewish Affirmations and Philosophical Reflections*, 8–19, *passim*.

God's promise that in Abraham's seed the gentiles would also share the blessings of the covenant. St. Paul's insistence that in Christ there was neither Jew nor Greek wove together strands common to the theology of both faiths. In Western Christendom thereafter the resistance to sectarianism by the established churches rested on claims to universality that lie at the heart of the New Testament and, in the apostolic reinterpretation of it, of the Old Testament as well. We ought not to be surprised, therefore, to find the idea of catholicity as prominent in the teachings of eighteenth-century Quakers as in those of the Anglican Society for the Propagation of the Gospel in Foreign Parts. John Wesley's inclusive notion of a world parish served his followers as well in the eighteenth century, when they were dissenting Anglicans, as in the nineteenth, when Methodists became the largest Protestant denomination in the United States.⁸⁶ During the latter century also, Rome's "missionary" bishops in America appealed to the idea of catholicity in resisting the formation of ethnic parishes and dioceses. These separate units were demanded—first by German priests and then by Polish, Italian, Czech, Slovak, French Canadian, Slovenian, Portuguese, and Spanish-American ones—on alternative "ecumenical" grounds: God was not interested in Americanization but in Christianization. Francis Hodur, who left the Roman communion, stood for catholic Polishness, while his countrymen who remained loyal to the existing structure stood for Polish Catholicism in a pluralistic church.⁸⁷

Similarly, the ethnic Greeks, the dominant group among the Eastern Orthodox, supported the universalist policies of the Ecumenical Patriarch of Constantinople. Though always Greek, the patriarchs were committed by their struggles for the leadership of Orthodoxy outside Eastern Europe to the ancient doctrine of "one city, one bishop" against the intentions of American Serbs, Rumanians, "Russians," Syrians, or Bulgarians to acknowledge only ethnic bishops who were loyal to the patriarchs of Belgrade, Bucharest, Moscow, Antioch, or Sofia. But in their devotion to the notion of ecumenicity the American Greeks were far behind the band of exiled Russian priests led by Georges Florovsky and his protégés, Alexander Schmemmann and John Meyendorff. Between the two world wars Florovsky made the tiny Paris hilltop where Saint Sergius Seminary and Nicholas Berdyaev were housed the intellectual and spiritual center for the theological revival of ecumenical Orthodoxy in Western Europe. After World War II Schmemmann and Meyendorff accomplished the same for Saint Vladimir's Seminary in Yonkers, New York.⁸⁸

With these illustrations in mind, the declarations of the religious leaders of American blacks, compounded of affirmations and renunciations of their

⁸⁶ Semmel, *Methodist Revolution*, 90, 97, 152–57; and Albert C. Outler, *John Wesley*, Library of Protestant Thought (Oxford, 1964), 20–21.

⁸⁷ Buczek, *Immigrant Pastor: The Life of the Right Reverend Lucyan Bójnowski*, 13, 15–16, 62–63; and William Galush, "The Polish National Catholic Church: A Survey of Its Origin, Development, and Missions," *Records of the American Catholic Historical Society of Philadelphia*, 83 (1972): 137–38, *passim*.

⁸⁸ For this story in more detail, see my "Refugee Orthodox Congregations in Western Europe, 1945–1948," 316–17, 321. Also see George H. Williams, "George Vasilievich Florovsky: His American Career (1938–1965)," *Greek Orthodox Theological Review*, 11 (1965): 9–12, 23–33, 100–06. On the "catholic" ideal in Uniate faith, see Nemec, "Ruthenian Uniate Church," 386–87.

oneness with white Christians, reflect both their reaction to racism and their awareness of the biblical teaching of a common humanity. The African Methodist Episcopal Church, which for generations followed both biblical and Methodist prescriptions in proclaiming the unity of the human race, was in this respect not one step ahead of the Church of God and Bible Prophecy or of Father Divine.⁸⁹

THIS EXTENDED SUMMARY OF THE RELATIONSHIP between religion and ethnicity in America demonstrates that we have now come to the point where anthropological, sociological, psychological, and historical perspectives on ethnicity can coalesce.⁹⁰ The volume recently edited by sociologists Nathan Glazer and Daniel P. Moynihan and the recent work of numerous anthropologists reveal the enrichment and diversification of theory and empirical analysis that has come from the comparative study of worldwide ethnic group relationships. Meanwhile, Josef Barton's *Peasants and Strangers* has signaled the growing preoccupation of younger historians of ethnicity with complexity, ambivalence, contradiction, and what I call—in conscious rejection of the metaphors of both melting pot and mosaic—kaleidescopic change. Scholars in all of these fields should abandon the notion that a set of fixed primordial realities lies behind the changing ethnoreligious relationships we are able to observe and analyze. That Heraclitus should replace Thales as our mentor will please those who find Alfred North Whitehead a modern culture hero. Perhaps also some may be pleased to discover that Moses, Jesus, and Paul were also prophets of process theology—men who called us away from simplistic notions of order, virtue, or psychic health and demanded that we deal with the real and mysteriously complex world of change in terms less doctrinaire and more compassionate than either religious or intellectual dogmatists have recently employed.

⁸⁹ Richard Allen, *The Life, Experience, and Gospel Labors of the Rt. Rev. Richard Allen, To Which Is Annexed the Rise and Progress of the African Methodist Episcopal Church . . .* (New York, 1960), 15–41, 70–71; and Benjamin T. Tanner, *An Apology for African Methodism* (Baltimore, 1867), 195.

⁹⁰ Edward Shils, "On the Comparative Study of the New States," in Geertz, *Old Societies and New States*, 12–15.

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The Continuous Shaping of America:
A Prospectus for Geographers and Historians

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GEOGRAPHY AND HISTORY are rooted in the basic stuff of human existence. As fields of study they are analogous, complementary, and interdependent. Their relationship is implied by such common terms as space and time, places and events—pairs that are fundamentally inseparable. What differentiates geography and history is the proportionate emphasis each gives to these terms. A corollary of such a view is that the two disciplines are a single species, a different breed, distinct from the sciences, even though they are linked by numerous bloodlines that many of our colleagues are wont to stress. Some historians and many geographers work as analytical scientists, but our fields, taken as a whole, have traditionally been expected to perform an ultimate task rather different from that assigned the sciences. We describe the entire world. We are at once comprehensive and naive; our concern is the whole course of human life on the whole expanse of the earth. Put in simplest terms, geography and history, unlike the social sciences, are a point of view: they are not the study of any particular set of things, but are a particular way of studying anything. Hence, the bewildering range of topics that characterize our literature. We bind these disparate topics together by a common approach—by a search for patterns of arrangements and relationships that allow us to describe and assess complex ensembles and sequences that themselves comprise different classes of phenomena.¹

But, although we may be kin, in America we have not been close—at least

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¹ This opening assertion is made to delineate the context of this prospectus rather than to engage in the chronic philosophic dispute about the nature of these two disciplines. I am aware that many historians and probably most American geographers regard themselves as social scientists. Over the past twenty-five years there has been a very strong move to define geography as a formal science; see, for example, Ronald Abler, John S. Adams, and Peter Gould, *Spatial Organization: The Geographer's View of the World* (Englewood Cliffs, N.J., 1971). But that movement is now perceptibly waning as its limitations as well as its contributions become better appreciated. Geography has been greatly enriched by this emphasis upon a more theoretically analytical approach, and much geographic research in the future will be undertaken within such frameworks. As the zealotry of the reformers has cooled, however, it is increasingly realized that all of their methods and all except their ultimate objectives can be encompassed within the venerable catholic tradition of the field. In the large literature on these disputations I have found a few studies to be espe-

not in the work of interpreting the geography and history of our own country. The evidence is obvious in our books. The best of our university texts, which purport to offer an overview of the major features of the character and experience of the United States, contain no significant sharing of perspectives, themes, concepts, or methods. One can only conclude that as fields we have paid little attention to one another (few of our graduate students have had even a single seminar in the other department—and, indeed, there is no department of geography at several of the leading centers for training in history).² There are, to be sure, atlases of history, which represent the application of the map, the most fundamental tool of the geographer, to the needs of the historian. The best of these offer a good deal more than the elemental “where” of specific events, but they demonstrate little real convergence of professional interests.³

Historical geographers are an exception for they are a guild that explicitly cultivates intermediate ground. The first substantial modern products of that field were two books by Ralph H. Brown: *Mirror for Americans: Likeness of the Eastern Seaboard, 1810* (1943) and *Historical Geography of the United States* (1948). Brown focused on “the geography of the past,” the reconstruction of the character of regions and landscapes from original documents, emphasizing the need to see geographical areas in the past as contemporaries saw them, for “men at all times have been influenced quite as much by beliefs as by facts.”⁴ These gracefully written books are landmarks of continuing value, but they were consciously limited in approach and coverage. They dealt with the formative periods of U.S. settlement and touch only occasionally the major themes and interpretations of historians.

Over the past thirty years, however, a literature in historical geography has emerged that explicitly links the two fields. The most notable body of work is that of the late Andrew Hill Clark and his students at Wisconsin, who have produced many monographs of solid scholarship. Clark played a leading role in establishing the international *Journal of Historical Geography* and initiated the Oxford Historical Geography of North America Series designed “to provide a small library of thematic studies” by professional geographers intended

cially lucid and congenial; see F. Lukerman, “Geography as a Formal Intellectual Discipline and the Way It Contributes to Human Knowledge,” *Canadian Geographer*, 8 (1964): 167–72; Cole Harris, “Theory and Synthesis in Historical Geography,” *Canadian Geographer*, 15 (1971): 157–72; and Leonard Guelke, “Problems of Scientific Explanation in Geography,” *Canadian Geographer*, 15 (1971): 38–53. On the fundamentals of the human experience with “place,” see E. Relf, *Place and Placelessness* (London, 1976); and Yi-fu Tuan, *Space and Place: The Perspective of Experience* (Minneapolis, 1977).

² There are numerous encouraging signs, however, and this article is a product of one, for it is an extensively recast version of “Geography and History: Prospectus for a Partnership” presented by invitation of the Program Committee to the session on “History and the Related Disciplines” at the 91st annual meeting of the American Historical Association. David Ward’s succinct assessment of approaches in historical geography derived from an analogous invitation from the Organization of American Historians to participate in a workshop on “New Approaches to History”; see Ward, “The Debate on Alternative Approaches in Historical Geography,” *Historical Methods Newsletter*, 8 (1975): 82–87.

³ For the best exhibit and a great collaborative effort by Charles O. Paullin and John K. Wright, see Paullin and Wright, *Atlas of the Historical Geography of the United States* (New York, 1932; reprint ed., Westport, Conn., 1975); for a widely distributed example, see Hilde Heun Kagan et al., *The American Heritage Pictorial Atlas of United States History* (New York, 1966); and, for an especially interesting one, see Theodore R. Miller, *Graphic History of the Americas* (New York, 1969).

⁴ Brown, *Historical Geography of the United States*, 3.

mainly for historians.⁵ Within the general literature of historical geography there are now some signs of genuine convergence, of historians and geographers drawing upon each other's concepts and tools and writing for both sets of readers. There are even instances of fruitful teamwork.⁶

Yet the literature does not contain, as far as I am aware, a coherent conceptual framework for the overall historical geography of the United States. I am thinking here of some sort of complement to historians' treatments of the full course of our history. Obviously, there can be no one best nor enduring way to do this overview; just as obviously, the project cannot be a simple distillation of or projection from specialized research studies. There must be a selection of themes and organizing concepts. In geography the logical complement to what I propose is Wilbur Zelinsky's *The Cultural Geography of the United States* (1973). His essay is, in effect, a richly annotated outline for an interpretative geographic study of the U.S. national character. What follows is a spare and tentative outline for an interpretative geographic study of our national experience. Although differing in objectives and emphases, both deal basically with the culture history and, therefore, with many common topics.

The perspectives of geography logically provide a number of possible approaches for such a prospectus. Historians may assume that an environmental approach, based either upon some sort of Braudelian fundament or upon the history of environmental perceptions and developments, would be the obvious central ground for a geographic framework.⁷ These should certainly be encouraged. But this prospectus is based more upon *area* than environment. It views the United States as a dynamic area—as a culture, society, and nation ever-changing in areal extent, structure, functions, and content and as an areal phenomenon that can be traced from its tiny and complex origins to its present enormous and complex macrocultural dimensions. It is a historical view but one manifestly different from that commonly taken by historians. Since the differences between geography and history are matters of emphasis, with no clear division of labor, it is a program as open to the historian as to the geographer.⁸

⁵ For the work of some of Clark's students, see James R. Gibson, ed., *European Settlement and Development in North America: Essays on Geographical Change in Honour and Memory of Andrew Hill Clark* (Toronto, 1978). For the thematic studies of the Oxford University Press series that have already been published, see D. Ward, *Cities and Immigrants* (1971); D. W. Meinig, *Southwest* (1971); R. C. Harris and J. Warkentin, *Canada before Confederation* (1964); D. McManis, *Colonial New England* (1975); J. Gibson, *Imperial Russia in Frontier America* (1976); H. Johnson, *Order upon the Land* (1976); and J. Jakle, *Images of the Ohio Valley* (1977).

⁶ See, for example, Harold M. Mayer and Richard C. Wade, *Chicago: Growth of a Metropolis* (Chicago, 1969); James T. Lemon and Gary B. Nash, "The Distribution of Wealth in Eighteenth-Century America," *Journal of Social History*, 2 (1968): 1–24; Joseph A. Ernst and H. Roy Merrens, "The South Carolina Economy in the Middle Eighteenth Century: A View from Philadelphia," *West Georgia College Studies in the Social Sciences*, 12 (Carrollton, Ga., 1973): 17–30, and "'Camden's Turrets Pierce the Skies!': The Urban Process in the Southern Colonies," *William and Mary Quarterly*, 3d ser., 30 (1973): 549–79.

⁷ John A. Jakle describes a number of possibilities; Jakle, "Time, Space, and the Geographic Past: A Prospectus for Historical Geography," *AHR*, 76 (1971): 1084–1103. Another major, emerging theme is the making of the American landscape, with an emphasis upon interpretations of American culture. John Brinckerhoff Jackson has been the most important catalyst in this field through the magazine *Landscape*, which he founded and long edited, and through his teaching at Berkeley and Harvard; also see his *American Space: The Centennial Years, 1865–1876* (New York, 1972).

⁸ In the enormous literature bearing upon the sequence of topics in this prospectus, the product of geographers is miniscule in comparison with that of historians, but it is of critical importance in demon-

WE MUST BEGIN IN EUROPE and try to make geographic sense out of the impetus for transatlantic venturing on the part of many different peoples. And we must ask two fundamental questions: (1) why do major cultural patterns and movements begin where they do (the problem of the “culture hearth”), and (2) how do they spread to other peoples and areas (a problem of “spatial diffusion”)? Seville and Lisbon were the first great seats of American enterprise. Located in areas of creative turbulence, they became the foci where a great tradition of imperial conquest forged in the reconquest of Iberia from the Moors converged with a great tradition of commercial seafaring developed in the many cosmopolitan centers of the Mediterranean. The movements of people and the functioning of institutions can be traced to the articulation of these systems in Andalusia and Estremadura and their reach outward to a succession of Atlantic islands, to the West African coast, to the West Indies, and to Brazil. Thus, we can regard the “Latin America” created therefrom as the continuation of a great geographic process initiated by an aggressive, expansive Western Christian culture centuries before.

Sebastian Cabot was only five years behind Christopher Columbus, and Bristol fishermen may have been well ahead of both. Yet serious English colonization only came decades later. There is, in fact, a fairly clear sequence in the initiatives for colonization that came in the wake of Iberian enterprise: first the Rochellais Huguenots, then the West Country English, Norman French, Londoners, and the Dutch. Ideally, we should map how the “idea of America” (not just the knowledge that a new land existed but a recognition of the opportunities it provided and an understanding of the means to seize them) was diffused from the Guadalquivir to the Thames and the Scheldt. The process and pattern, of course, were complicated. The linkages involved the migration of men as well as the routine transmission of commercial information, the calculated spying and archival searches of a Hakluyt as well as everyday gossip among seamen. We need to understand the basic circulation system of the Western European economy and to take into account events that intensified contacts between peoples—those, for example, between the Huguenots and the English and between the English and the Dutch. We should pay close attention to changes in social geography emanating from the politicoreligious turbulence in France and Flanders and affecting many communities in southeastern England and Holland from which important American ventures emanated. This maritime arena of Northwest Europe was another culture hearth for American enterprise. It, too, was an area of social and political turbulence but, without the institutions and fresh experience of a *Reconquista*, French, English, and Dutch colonization was far more experimental, more diverse in type, and more varied and uncertain in result than the Iberian counterpart, and the America created out of this corner of Europe was necessarily more an invention than a continuation.⁹

This geographic view tries to see specific actions always within the context of the spatial systems that linked these two great nuclear areas, the states that

strating particular approaches and methods. I have, therefore, confined my citations very largely to works by historical geographers.

⁹ James E. Vance, Jr. has, however, made a strong claim for the significance of thirteenth-century

occupied them, and the regions within those states. These links, in turn, became part of a vast North Atlantic system that increasingly bound Western Europe, West Africa, and the Americas together. Following initial reconnaissance, "points of attachment" were established that anchored this new system to the American side. Some were commercial outposts, and others were colonizations; all were intrusions upon Amerindian societies. The outposts lured the Indians into becoming the outermost participants in the expansive European commercial system, as at Quebec; the colonizations expelled the Indians to make way for the sudden transoceanic extension of the European culture area, as along the James River. In either case the common pattern was the establishment of a headquarters town more or less central to a claimed segment of the coast.¹⁰ As this oceanic system began to pulsate with an increasing volume and diversity of people and goods, patterns of change spread inward on each continent. Human geographies were altered radically in America, extensively in Africa, and more subtly transformed in Europe where the impact of American enterprise slowly shifted some of the proportions and directions of the established spatial systems.¹¹ Such a geographic view shifts the emphasis from discovery of the New World by the Old to the European initiation of a sudden and harsh encounter between three Old Worlds and the progressive integration of them into a single New World.

THOSE PRIMARILY INTERESTED IN THE NORTH AMERICAN SECTOR of this vast region must trace what unfolded from the many points of attachment within it. Some of these points were ephemeral, and others that endured were of little consequence; but some became nuclei of discrete colonization areas that lasted long enough to leave a distinctive imprint on the American seaboard and on the course of American development. Geographical analysis should help account for the differences in these early results, but our main focus must be on the geographic character of the main nuclei. Each nucleus can be analyzed as a *spatial system*—a network of nodes and links that channeled the movements of people, goods, and messages within the bounds of a defined territory. The most obvious components of the system were the political

colonization in Aquitaine as a precedent for that in America; Vance, *This Scene of Man: The Role and Structure of the City in the Geography of Western Civilization* (New York, 1977), esp. 176–200. Ireland is often cited as a laboratory for American colonization, but major efforts at expansion were simultaneous (Munster and Roanoke, Ulster and Virginia). Furthermore, despite important connections between the two areas in people and policies, Ireland might also be considered an "intervening opportunity" that absorbed energies, manpower, and capital.

¹⁰ For a spatial model of the economic aspects of this transoceanic interaction, see James E. Vance, Jr., *The Merchant's World: The Geography of Wholesaling* (Englewood Cliffs, N.J., 1970), esp. chap. 7. For an abstract of an attempt to include other social and cultural topics, see D. W. Meinig, "Spatial Models of a Sequence of Trans-Atlantic Interactions," in 23rd International Geographical Congress, Section 9, *Historical Geography* (Moscow, 1976): 30–35; and, for a pertinent variation, see Carville V. Earle, "The First English Towns in North America," *Geographical Review*, 67 (1977): 34–50.

¹¹ See, for example, W. Iain Stevenson, "Some Aspects of the Geography of the Clyde Tobacco Trade in the Eighteenth Century," *Scottish Geographical Magazine*, 89 (1973): 19–35; and W. G. Handcock, "Spatial Patterns in a Trans-Atlantic Migration Field: The British Isles and Newfoundland during the Eighteenth and Nineteenth Centuries," in Brian S. Osborne, ed., *The Settlement of Canada: Origins and Transfer* (Kingston, Ontario, 1976), 13–45.

subsystem (the locations of the governor and subordinate officers and the logistical network of associated institutions) and the economic subsystem (the locations of production, manufacture, and consumption, routes of transport, and points of exchange). Each nucleus can also be analyzed as a *cultural landscape*—the result of the domestication of a particular kind of country by a particular group of immigrants that imprinted the area with a geometry, morphology, and architecture of settlement; introduced a selection of crops, animals, technology, and economic activities; and created particular patterns of ecological alteration. And each nucleus can be analyzed in terms of *social geography*—the distribution and demographic character of its population, the locations of important social groups (however identified), and the basic social institutions and contexts (such as the village, market town, county, plantation, tenanted estate, freehold farm, and so forth) that served as matrices for the emergence of distinctive local societies.

These nuclei may be thought of as the basic units in the geographical growth of the American nation. We can identify about twenty that had some direct bearing on the eventual United States, and we must trace their geographical history. They differed greatly in their power to expand, and it is necessary to examine carefully the results arising from the inevitable encounters between adjacent nuclei. To what extent did these encounters lead to stable separation, conflict, penetration, mixture, merger, or absorption? Expansion from the most vigorous nuclei created territorial units on a new scale, which I shall call regions.¹²

In this view, therefore, the most important task in the historical geographic study of colonial America is to define as clearly as possible this sequence of territorial formation from points to nuclei to regions on the North American seaboard and to describe the changing geography of each in terms of spatial systems, cultural landscape, and social geography. Along the Northeastern coast, for example, we can trace the emergence over a century and a half of what may be called Greater New England. In general Greater New England was characterized by a strong Puritan imprint upon landscape and society, upon the scale, design, and sense of community; it was a distinct field of population movements and possessed a network of social relationships; it was a system of interaction and Boston was its principal focus and seat of authority and influence. But it was not a homogeneous surface nor a simple structure. At least eight early nuclei were directly involved in its formation: Acadia-Nova Scotia, the Piscataqua, Massachusetts Bay, Plymouth, Rhode Island, the Connecticut Valley, New Haven, and the Hudson Valley and Estuary. These varied greatly in cultural individuality and power of expan-

¹² I know of no study that exemplifies this approach on the same scale. R. Cole Harris and John Warkentin include much material on these three categories of features; Harris and Warkentin, *Canada before Confederation: A Study in Historical Geography* (New York, 1964). Various elements and subsystems are treated in some historical geographic works; see, for example, James T. Lemon, *The Best Poor Man's Country: A Geographical Study of Early Pennsylvania* (Baltimore, 1972); Carville V. Earle, *The Evolution of a Tidewater Settlement System: All Hallow's Parish, Maryland, 1650-1783* (Chicago, 1975); and Peter O. Wacker, *Land and People: A Cultural Geography of Preindustrial New Jersey—Origins and Settlement Patterns* (New Brunswick, N.J., 1975).

sion. Massachusetts Bay absorbed Plymouth but not the Piscataqua, despite comparable general processes and pressures; and Connecticut absorbed New Haven, but Rhode Island persisted with more than just political and religious independence. Nova Scotia, New York, and East Jersey were alien border areas vigorously invaded by Yankee farmers and businessmen but not fully dominated by them. Newfoundland, at the farthest reach, received strong economic penetration but without strong cultural effect. Inland from the core area of lowland Massachusetts and Connecticut was an expanding frontier different not only because of its remoteness but also because New England culture itself had changed by the time the frontier was being colonized. Boston was the principal focus of the region, but Newport, Providence, Portsmouth, New Haven, New York City, and Halifax were competing centers in a complex of partly subordinate and partly overlapping spatial systems. A further complication—the varied perceptions of “New England,” its character and extent, in the minds of contemporaries—provides an important dimension, but the value of the region as an analytical concept is not necessarily proportionate to the strength of such regional consciousness.

Such a view relegates the geopolitical *colony*, a territorial jurisdiction, to no more than a subsystem. Although important, often decisive, in shaping much else, such colonies cannot serve as the fundamental geographical entities, because spatial systems, landscapes, and social geographies are not necessarily concordant with their bounds (nor, indeed, are these three always concordant with one another). In the vast literature on the colonies historians have dealt with many topics relevant to the needs of this approach, but there is much that has never been investigated. One cannot simply piece together this kind of historical geography. Indeed, the specific identification of these geographical nuclei and regions for the American seaboard from Newfoundland to Louisiana can be little more than an assertion until a good deal of research has been undertaken within this framework.¹³

Regional formation leads logically and historically to national formation. There is a considerable literature in social science on “nation-building.” Karl Deutsch’s influential formulation has a strong geographical emphasis involving in his eight-part model such processes as the development of core areas, the growth of towns, the extension of a communications grid, and an increase in social mobility, all of which lead to the knitting together in ever tighter networks of communication and complementarity an ever larger proportion of the population.¹⁴

Viewed in this context the American Revolution is a convulsive, somewhat chaotic event that created a new state but not a closely integrated nation. The geographer’s interest is not primarily in the momentous political issues and

¹³ Douglas R. McManis has touched on several topics pertinent to this approach but has not focused on the tracing of this kind of geographical development; see his *Colonial New England: A Historical Geography* (New York, 1975). The paucity of geographical studies on New England contrasts starkly with the volume of historical works.

¹⁴ Deutsch, “The Growth of Nations: Some Recurrent Patterns of Political and Social Integration,” *World Politics*, 5 (1953): 168–95.

battles but in how the human geography of the American seaboard was changed and how the processes of territorial integration were affected. The first involves an inventory of changes between roughly 1770 and 1790, and that splendid new collaboration among historians, geographers, and cartographers, *The Atlas of Early American History* (1976) under the editorship of Lester Cappon, supplies much pertinent information. The second is more elusive, since it entails the identification of expanding patterns of contact, the diffusion of ideas, different patterns of acceptance, the exodus of resisters, the improvement of communications facilities, and the growth in interregional complementarity.¹⁵

Certainly the United States did not begin with a single core area. The lack of an obvious vital center bedeviled British strategists all through the war, and the concept of a federation of equal states worked against the quick emergence of any one as central and superior. Furthermore, the opening of the western lands to settlement led to a broad dispersal of the population and the creation of new regional systems, anchored in but grossly distended from the major centers of the seaboard. The broadly latitudinal reach of New Englanders, Pennsylvanians, and Virginians across the Appalachians into the New West was a pattern emphasized by Frederick Jackson Turner and is well known to historians. But this westward movement cannot yet be readily understood in terms of spatial systems, cultural landscapes, and social geographies. The geographic view is less concerned with episodic political expressions of sectionalism than with (1) such measurable cultural evidence as vernacular architecture,¹⁶ town plans,¹⁷ agricultural systems,¹⁸ church congregations,¹⁹ and social institutions,²⁰ and (2) such systems of circulation as facilities and services in the sequence of turnpikes, canals, railroads, and telegraph lines.²¹

¹⁵ Fifty years ago Michael Kraus provided a very useful introduction to interpersonal contacts; see his *Intercolonial Aspects of American Culture on the Eve of the Revolution* (New York, 1928). More recently, Richard L. Merritt has explored some measures of interregional interaction; Merritt, *Symbols of American Community, 1735-1775* (New Haven, 1966). And historians have provided many of the pieces, but we are still far from having anything like a geography of the revolutionary era focused on the nation-building process.

¹⁶ Peirce F. Lewis, "Common Houses, Cultural Spoor," *Landscape*, 19 (1975): 1-22; Fred B. Kniffen, "Folk Housing: Key to Diffusion," *Annals of the Association of American Geographers* [hereafter *AAAG*], 55 (1965): 549-77, and Kniffen and Henry Glassie, "Building in Wood in the Eastern United States: A Time-Place Perspective," *Geographical Review*, 56 (1966): 40-66; Hubert G. H. Wilhelm, "The Pennsylvania-Dutch Barn in Southeastern Ohio," in H. J. Walker and W. G. Haag, eds., *Man and Cultural Heritage: Geoscience and Man*, 5 (Baton Rouge, 1974): 155-62; and Douglas K. Meyer, "Diffusion of Upland South Folk Housing to the Shawnee Hills of Southern Illinois," *Pioneer America*, 7 (1975): 56-66.

¹⁷ Edward T. Price, "The Central Courthouse Square in the American County Seat," *Geographical Review*, 58 (1968): 29-60.

¹⁸ J. E. Spencer and Ronald J. Horvath, "How Does an Agricultural Region Originate?" *AAAG*, 53 (1963): 74-92; and Paul C. Henlein, "Shifting Range-Feeder Patterns in the Ohio Valley before 1860," *Agricultural History*, 31 (1957): 1-12. For an example of cultural analysis, see Terry G. Jordan, "Early Northeast Texas and the Evolution of Western Ranching," *AAAG*, 67 (1977): 66-87.

¹⁹ Edwin Scott Gaustad, *Historical Atlas of Religion in America* (New York, 1962); and Robert D. Mitchell, "The Presbyterian Church as an Indicator of Westward Expansion in 18th-Century America," *Professional Geographer*, 28 (1966): 293-99.

²⁰ Fred B. Kniffen, "The American Agricultural Fair: Time and Place," *AAAG*, 41 (1951): 42-57.

²¹ Allan R. Pred, *Urban Growth and the Circulation of Information, 1790-1840* (Cambridge, 1973). For a different illustration of the functioning of the system, see G. F. Pyle, "The Diffusion of Cholera in the United States in the Nineteenth Century," *Geographical Analysis*, 1 (1969): 59-75.

There remains the major task of defining the social geography of major communities and districts²² and the personal and institutional links that bound them into larger networks.

Great complexities abound because we have yet to sort out several sets of emerging spatial systems: those forming around new cities in the West, linear axes along the Ohio River and later the Lower Lakes,²³ interregional systems bound to the great Atlantic seaports, and an emerging overall national network. All of these are interlocked but not in simple regional or hierarchical patterns. Similarly, we must trace the persistence of old and the development of new cultural areas as expressed in landscape, social geography, and overt political behavior, as well as the continual emergence of a national culture stamping a degree of uniformity upon all areas. The Ohio Country was a kind of national hearth where peoples converged from several seaboard regions (but the geography of that convergence has not been systematically assessed) as well as directly from Europe. The area served as a laboratory for the initiation of new national policies dealing with such fundamentals as land survey and disposal²⁴ and the formation of new states.

That nation-building and the growth of a Southern regional consciousness occurred simultaneously for several decades is commonly appreciated, but the topic has not undergone careful geographical analysis. We need to know much more about the formation and the content of both the nation and the region. The historical literature on the South is laced with geographical terms: Upper South, Lower South, Upland South, Lowland South, Border South, Deep South, Tidewater, Frontier, Plantation South, Black Belt. The discordance and confusion in concepts and areas here can best be resolved by careful study of the processes and results of regional formation, beginning with initial nuclei (including antecedents in the West Indies) and leading methodically to a consideration of the Confederacy as an incipient national state. There are, of course, great complexities in this task. At the very least, we

²² For a seminal statement of the need and a context for such a study, made over twenty years ago, see Conrad M. Arensberg, "American Communities," *American Anthropologist*, 57 (1955): 1143-60; and, for a powerful display of what systematic study of the individuality of communities can yield, see Daniel J. Elazar, *Cities of the Prairie: The Metropolitan Frontier and American Politics* (New York, 1970). For some recent attempts to map the migration patterns pertinent to the cultural character of districts and communities, see Douglas K. Meyer, "Native-Born Immigrant Clusters on the Illinois Frontier," *Proceedings of the Association of American Geographers*, 8 (1976): 41-46; William A. Bowen, "American Ethnic Regions, 1880," *ibid.*, 44-46; Albert J. Larson, "Northern Illinois as New England Extended: A Preliminary Report," *Pioneer America*, 7 (1975): 45-51; and, especially, John C. Hudson, "Migration to an American Frontier," *AAAG*, 66 (1976): 242-65.

²³ Michael P. Conzen, "A Transport Interpretation of the Growth of Urban Regions: An American Example," *Journal of Historical Geography* [hereafter *JHG*], 1 (1975): 361-82; Edward K. Muller, "Selective Urban Growth in the Middle Ohio Valley, 1800-1860," *Geographical Review*, 66 (1976): 178-99; Fred Lukermann, "Empirical Expressions of Nodality and Hierarchy in a Circulation Manifold," *East Lakes Geographer*, 2 (1966): 17-44; and Margaret Walsh, "The Spatial Evolution of the Mid-Western Pork Industry, 1833-75," *JHG*, 4 (1978): 1-22. For a more theoretical statement, see Edward K. Muller, "Regional Urbanization and the Selective Growth of Towns in North American Regions," *JHG*, 3 (1977): 21-39.

²⁴ William D. Pattison, *Beginnings of the American Rectangular Land Survey, 1784-1800* (Chicago, 1957); Norman J. W. Thrower, *Original Survey and Land Subdivision: A Comparative Study of the Form and Effect of Contrasting Cadastral Surveys* (Chicago, 1966); and Hildegard Binder Johnson, *Order upon the Land: The U.S. Rectangular Land Survey and the Upper Mississippi Country* (New York, 1976).

must sort out several sets of patterns: the cultural surface resulting from the initial peopling of the area,²⁵ the development of major production districts,²⁶ the emergence of commercial nodes and hinterlands,²⁷ the formation of states and geopolitical balances within them, and of region-wide networks of influential persons with common interests.²⁸ If we could describe and map these features in some detail we could get underneath that great load of generalizations about "The South" and assess much more effectively the degree to which such concepts as "agrarianism," "biracialism," "slavery," "folk culture," "King Cotton," and "sectionalism" actually apply district by district, region by region within this portion of the nation, and we should be able to assess much more acutely several important more closely focused topics, such as the diffusion of the idea of secession, the detailed pattern of rupture along a broad belt from Delaware to Kansas, and the geopolitical character and problems of the Confederacy.

As for the relationships between the South (or Souths) and the nation, our threefold geographical examination of cultural landscapes, spatial systems, and social geography can provide a fresh and valuable addition to the voluminous political literature. Vernacular landscapes, for example, display ordinary traditions, tastes, and values as expressed in tangible cultural features. Comparative study of domestic, rural, town, civic, and commercial architectures and morphologies in the main regions of the United States in any era can provide important measures of the relative significance and trends of national versus distinctly regional styles, a subtle but fundamental measure of the emergence of a common culture.²⁹ From fragmentary evidence it appears that the landscape of antebellum America was, as we might expect, ambivalent: broadly similar or converging civic styles and persistently different domestic, rural, and, to some degree, commercial styles.

That Northern and Southern societies were diverging in important characteristics, especially in regard to heterogeneity of peoples (and therefore sub-societies) and diversity of economies, is a commonplace, but, again, the geographical view requires a much more careful definition of the variations in such things district by district. Examination of the development of spatial systems should give special attention to key strands and points of inter-regional linkages. We can at least document the physical facilities and much of the scheduled services provided, even if the detailed nature and volume of

²⁵ What we need is a geographical refinement of Clement Eaton's excellent overview; see his *The Growth of Southern Civilization, 1790-1860* (New York, 1961). The meticulous descriptive and broader interpretive works of Milton S. Newton, Jr. offer a direction; see, for example, Newton, "Cultural Preadaptation and the Upland South," in Walker and Haag, *Man and Cultural Heritage*, 143-54, and "Folk Material Culture of the Lower South, 1870 to 1940," in Lucius F. Ellsworth and Linda V. Ellsworth, eds., *The Cultural Legacy of the Gulf Coast, 1870-1940* (Pensacola, 1976).

²⁶ Sam B. Hilliard, *Hog Meat and Hoeecake: Food Supply in the Old South, 1840-1860* (Carbondale, Ill., 1972).

²⁷ Robert B. Mitchell, *Commercialism and the Frontier: Perspectives on the Early Shenandoah Valley* (Charlottesville, 1977).

²⁸ For the "literati" and the role of the county seat in Southern society, see Milton B. Newton, Jr., "Settlement Patterns as Artifacts of Social Structure," in Miles Richardson, ed., *The Human Mirror: Material and Spatial Images of Man* (Baton Rouge, 1974), 339-61.

²⁹ Zelinsky, *Cultural Geography of the United States*, 85-94.

actual movements of goods and people may be largely unobtainable. Close study of the operations of key business, institutional, and professional connections in the main entrepôts can give further evidence of the degree to which the United States was actually functioning as a national system.³⁰

As in the Revolution, the geographer is not primarily interested in the Civil War as a cataclysmic national event but rather in its longer-term effects upon the geography of the nation. Secession, a disintegration of territory, is a geographical process. Study of the South and the nation as complex and continuously developing landscapes, systems, and geographies should provide a far richer context for consideration of the diffusion and differential response to the idea of secession. As for the war itself, the geographer's emphasis is not upon strategies and battles but upon enduring impacts on areas: displacements of population, construction and destruction of important facilities, and initiation and demise of economic activities. For the postwar period we must focus upon two topics, neither of which fits neatly into the usual framework of "Reconstruction." One topic is the reorganization of the Southern cultural landscape, especially alterations of the countryside in patterns of residences, services, field systems and other features involved in the freeing of slaves, and the implementation of the share-cropping system as well as analogous alterations of towns and cities under the gradual, inexorable pressures toward racial segregation. Studies now underway at micro-geographic levels will eventually provide the basis for more accurate regional assessments.³¹ A second major topic is the way in which the nation was stitched back together—involving the re-establishment of old links and the creation of new ones and tracing the penetration of the South by Northerners in various roles, the detailed geography of Northern control over Southern resources and facilities, and its impact upon communities. Much of this last topic can be illuminated by use of an imperial model of geographic change following military conquest. That model can, however, be most effectively illustrated in its various guises in the history of our westward expansion.

The United States was created by massive aggression against a long succession of peoples. That this movement was piecemeal, largely uncoordinated, at times and in some aspects even unconscious does not alter the destruction or displacement and deep deformation of all societies in its path. Those earliest outposts on the Atlantic coast mark the beginnings of a pattern of penetration of indigenous societies, attaching them to the European system through strands of commerce and the paths of agents and initiating an intense phase of culture change.³² Some of these societies remained client states on the frontier

³⁰ Michael P. Conzen, "The Maturing Urban System in the United States, 1840-1910," *AAAG*, 67 (1977): 88-108.

³¹ John Kellogg, "Negro Urban Clusters in the Postbellum South," *Geographical Review*, 67 (1977): 310-21; Peter C. Smith and Karl B. Raitz, "Negro Hamlets and Agricultural Estates in Kentucky's Inner Bluegrass," *Geographical Review*, 64 (1974): 217-34; and John P. Radford, "Race, Residence, and Ideology: Charleston, South Carolina in the Mid-Nineteenth Century," *JHG*, 2 (1976): 329-46.

³² See Arthur J. Ray, *Indians in the Fur Trade: Their Role as Hunters, Trappers, and Middlemen in the Lands Southwest of Hudson Bay, 1660-1870* (Toronto, 1974); and David Wishart, "Cultures in Co-operation and Conflict: Indians in the Fur Trade in the Northern Great Plains, 1807-1840," *JHG*, 2 (1976): 311-28.

for some time, others negotiated a withdrawal westward, but eventually all felt the relentless power of the expansion of European peoples. For a long while the most common practice was not the classic one of imperial conquest and subsequent exploitation of a captured people and its territory, but a more extreme one of expulsion.³³ Such a practice inevitably sent waves of geopolitical and social disruption radiating out ahead of the margins of actual control and colonization. Eventually, most of the survivors of the displaced societies became encapsulated on reservations.

On the Gulf Coast and in the Far West the more classic model appears in various degrees and guises. There is not space to describe this classic model in detail, but its important geographical processes include (1) geopolitical reorganization, which involves creating administrative territories, seating authority (often in a new capital), positioning military forces and logistic facilities, and imposing a system for the survey and allocation of lands; (2) economic change, which involves acquiring by force, politics, or purchase selected lands and resources, initiating new economic activities and perhaps suppressing the old, and creating new centers and networks and redirecting commercial connections; and (3) demographic and social change, which involves introducing agents and colonists among the indigenous peoples, displacing or subordinating the native population as expressed in residential and occupational segregation and social stratification, and continual pressing for cultural change as revealed by the presence of cultural institutions such as schools and churches and by the adoption of dominant modes of dress, equipment, and architecture.

The result is the creation of a new human geography, not out of the simple extension of the patterns of the expanding society as in the frontier model but a complex evolving composite of those of the invader and the invaded. In practice, the agents of expansion, proportionate powers, and results will vary greatly from region to region; but, if we study the affected Indian societies, the Creole Coast, Texas, New Mexico, Arizona, the Californias, the Mormon refugee commonwealth, the British American fur trade network, Hawaii, and Alaska in such imperial terms, we could establish a context for analysis and interpretation. There are great practical advantages to an imperial model over any frontier model: it includes the people on both sides of the frontier, and wherever these peoples persist together it allows us to follow their continuing interrelationships. Unlike the Turnerian frontier, imperialism in the sense of manifold pressures of a dominant national culture upon captured regional cultures does not end (except by annihilation or complete assimilation)—as many such groups defined in racial, cultural, or ethnic terms have reminded us lately.

³³ Francis Jennings, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill, 1975), esp. app.: "The Formative Period of a Large Society: A Comparative Approach," 327–35; D. W. Meinig, "A Macrogeography of Western Imperialism: Some Morphologies of Moving Frontiers of Political Control," in Fay Gale and Graham H. Lawton, eds., *Settlement & Encounter: Geographical Studies Presented to Sir Grenfell Price* (Melbourne, 1969), 213–40; and Carville V. Earle, "Reflections on the Colonial City," *Historical Geography Newsletter*, 4 (1974): 1–17, esp. Table 1.

In the five or six decades following the Civil War the United States also developed into a quasi-imperial structure on a broader scale. The subordination of the South and the West to the economic, political, and cultural power of the Northeast is an old theme in American history. But, again, it has never been studied as a major geographical topic. This era may be seen as the time when the geographical morphology of the American nation as a whole most clearly reflected a relatively simple structure of *core* and *domain*, as those terms have been defined in the literature of cultural geography.³⁴ Since I have elsewhere set forth in considerable detail a framework for the consideration of the changing relationships between core and domain as they pertain to regions of the American West through the whole course of national history, I will not repeat that here. And the position of the South is sufficiently analogous to be reasonably clear in such context.³⁵

LET US, INSTEAD, GIVE SPECIAL ATTENTION to the American *core*. The term is, of course an abstraction—an efficient means of reference to a critically important imbalance in the overall human geography of a culture, nation, or state. The concept refers to an areal concentration of elements basic to the cultural character and functioning of the society. The concept is not normative but heuristic, useful for historical and cross-cultural comparative analyses and important as a means of focusing attention upon critical questions as yet little studied. From roughly 1870 to 1930 it is easy to recognize a belt of country aligned more or less along a New York City–Chicago axis as a distinct region. Judged by productivity, value added, employment, and income and by density, intensity, and diversity, this area was a major differentiation within the national economic and demographic surface. Maps of facilities and services also show it as the focus of the national transportation and communication systems.³⁶ Core areas are also defined in terms of authority, as the seats of political, economic, and cultural power; such authority is wielded from points within some sort of spatial system. In fact, these systems are extremely complicated and interrelated and at best we can only hope to identify a few illustrative patterns. That Wall Street and the great Chicago commodity markets and mercantile houses were the symbolic centers of economic power during this era is commonly understood. We can plot the locations of the headquarters of the largest corporations, but we need to do considerably more to gain insight into the shaping of the spatial economy. Corporations whose operations extend beyond a single place can be analyzed as hierarchical

³⁴ D. W. Meinig, "The Mormon Culture Region: Strategies and Patterns in the Geography of the American West, 1847–1964," *AAAG*, 55 (1965): 191–220, and "Cultural Geography," in *Introductory Geography: Viewpoints and Themes*, Commission on College Geography Publication no. 5 (Washington, 1967), 97–103.

³⁵ D. W. Meinig, "American Wests: Preface to a Geographical Interpretation," *AAAG*, 62 (1972): 159–84, and *Imperial Texas: An Interpretive Essay in Cultural Geography* (Austin, 1969).

³⁶ For a brief overview of the emergence of this core, see David Ward, *Cities and Immigrants: A Geography of Change in Nineteenth-Century America* (New York, 1971). For a detailed description of the emergence of one of the fundamental elements in this pattern, see Kenneth Warren, *The American Steel Industry, 1850–1970: A Geographical Interpretation* (Oxford, 1973). And, for a seminal article on the concept of a core area and the U.S. pattern in the mid-twentieth century, see Edward L. Ullman, "Regional Development and the Geography of Concentration," *Papers and Proceedings, Regional Science Association*, 4 (1958): 179–98.

spatial systems, identifying the locations where basic decisions affecting investment, purchasing, employment, and marketing are made.³⁷ Fundamental to the overall national system are the locations of intelligence centers, where economic information is collected, processed, and generated and from which it is diffused.³⁸ To define such features for a few critical industries and institutions (such as financial houses, stock exchanges, commodity markets) at a sequence of dates would greatly increase our grasp of the developing geography of economic power, of the emergence of this core area, and of the ways in which and the degree to which the country was becoming more centralized as a national system.

Cultural power is a more elusive but nonetheless essential concept. Where are the major seats of influence upon national social patterns? Schools provide one way to get at this topic. They can be studied to ascertain the informal hierarchy of academic prestige as well as to identify those most important in training leaders of major corporations and institutions. The concept of cultural power leads into the complex subject of the "representativeness" of the basic characteristics of a culture. Are there spatial gradations of "Americanness"? In both popular and scholarly opinion some areas are often regarded as more "typically American" than others. We are immediately confronted here with the differences between what a society itself accepts as its identifying marks and what might be recognized as such by a reasonably dispassionate anthropologist. The former tends to focus on the prestigious and the ideal, the latter upon the common and the average; and the resulting judgments may be very discordant. As an example in social geography the symbolically ideal "American" population was homogeneous during this era in terms of language, dress, and many basic values and attitudes. It was a time when white, Anglo-Saxon Protestants dominated the institutions that shaped our national ideals and self-image, and there were powerful pressures upon the population to accept their standards. Yet the American population had always been diverse and was becoming increasingly so, in part through some further regional differentiation and especially from the great numbers of immigrants. This growing diversity was increasingly evident in the Northeast core, whereas some predominantly WASP Middle Western towns and small cities (there were not many) displayed better than the Northeast this symbolic "Americanness." We need geographic assessments of the characteristics and the bases of such symbols as well as careful delineation of actual diversities, place by place.³⁹

³⁷ Gunter Krumme, "Toward a Geography of Enterprise," *Economic Geography*, 45 (1969): 30-40; and Robert McNee, "Functional Geography of the Firm, with an Illustrative Case Study from the Petroleum Industry," *Economic Geography*, 34 (1958): 321-37.

³⁸ Allan R. Pred, "Urban Systems Development and the Long-Distance Flow of Information through Pre-electronic Newspapers," *Economic Geography*, 47 (1971): 498-524. Also see W. R. Code, "The Spatial Evolution of Financial Communities in Canada," Twenty-third International Geographical Congress, Section 9, *Historical Geography* (Moscow, 1976), 46-53; and Allan R. Pred, *Major Job-Providing Organizations and Systems of Cities*, Commission on College Geography, Resource Paper no. 27 (Washington, 1974).

³⁹ For a model geographic study of an immigrant group (Dutch Reformed) adapting to the opportunities and pressures of America, see Elaine M. Bjorklund, "Ideology and Culture Exemplified in Southwestern Michigan," *AAAG*, 54 (1964): 227-41.

Although largely undocumented geographically, the Northeast core defined the standard and provided the materials and the agents for an American way of life during this era. Our domestic, commercial, and recreational landscapes, as well as our styles of dress and social manners, came out of specific “power centers” in that part of the country. The first significant modification of this general regional dominance was made in the realm of cultural power. Beginning in the 1920s Southern California emerged as the culture hearth for a new American “lifestyle.” From there, primarily, has spread a powerful influence upon the kinds of houses we live in, the clothes we wear and activities we pursue, the design of our suburbs, shopping centers, and highways—upon the “car culture” that has so profoundly reshaped our geography along with our lives. It was the first region external to the Northeast core to have a national impact upon American culture. Southern California did not, however, become the seat of great economic or political power, and, except for this cultural shift, there was little modification of the basic national structure until after World War II. Over the past thirty years, of course, massive shifts in population and economic activities have occurred, and revolutionary innovations in transportation and communications. The gross quasi-imperial regional structure no longer exists in any clear sense. America now has a multi-metropolitan structure bound ever more closely together by interstate highways, air service, and instantaneous communication facilities.⁴⁰ We can document the major patterns of interregional population shifts, even if we have difficulty tracing cumulative movements district by district. Now we can better document geographic changes in major economic activities and, perhaps, the moves of important personnel. What we still seek to understand are key changes in the way the United States functions as a national system and in the cultural and social character of its landscapes and communities. The rise of the “Sunbelt,” the “Southernization of the United States,” and the “Americanization of Dixie” are themes prominent in current popular literature.⁴¹ Such journalistic interpretations cry out for careful assessment, and this can best be done, I believe, within a context that allows us at least to envision, even if not yet very clearly understand, such interregional shifts in the national balance as part of the continuous record of interaction, movement, and acculturation from our colonial beginnings to the present.⁴²

A LOGICAL EXTENSION OF THESE DEVELOPING PATTERNS brings us to a last topic that is even grander in scale and complexity. Examining the creation of America within an imperial framework compels us to follow that expansion wherever it may lead. Extension of American authority over parts of the

⁴⁰ John Borchert, “America’s Changing Metropolitan Regions,” *AAAG*, 62 (1972): 352–73.

⁴¹ Kirkpatrick Sale, *Power Shift: The Rise of the Southern Rim and Its Challenge to the Eastern Establishment* (New York, 1975). (The last two phrases are the subtitle and title respectively of a book by John Egerton [New York, 1974].)

⁴² For a stimulating essay on a major theme, see James E. Vance, Jr., “California and the Search for the Ideal,” *AAAG*, 62 (1972): 185–210.

western Pacific, over the Caribbean, and, briefly, over western Germany and the whole of Japan is clear enough as a simple geopolitical pattern, but the impress of American culture on those areas is as yet only vaguely grasped. And, of course, formal occupations represent only a small portion of the areas significantly affected by the enormously creative and aggressive power of American culture. America's cultural impingement upon other parts of the world is a fact of fundamental significance. It is one of the notorious realities of the twentieth century but I know of no concerted effort to bring this topic into coherent focus. The subject is, of course, immensely complex and difficult, but so are most of the great topics of geography and history. As a practical matter, one can only work methodically on some part of it with the most effective tools available, while retaining some vision of the overall context.

We can conveniently divide the topic into two broad kinds of impact: (1) the exercise of American control over facilities, institutions, resources, agents, and employment in foreign lands; and (2) the inducement, overtly or covertly, of measurable cultural change in other societies. The latter is, of course, the more difficult to assess, and we must seek the help of anthropologists, sociologists, and others to establish some order of significance among the many American elements and influences that have penetrated other cultures. Generally, the most important—changes in social values, attitudes, and behavior—are the most difficult to measure. We would have to start with a few selected items that we consider are diagnostic of wider changes. Geographically, we might well start with a few rather obvious areas. Puerto Rico and the Philippines would seem to be excellent laboratories for testing our concepts and sharpening our skills in mapping the spread (and, in the Philippines in some cases, now the retreat) of various indicators of change.⁴³ Even elementary comparative studies of just a few kinds of American penetration of Mexico, Quebec, and English-speaking Canada would be a great step forward.⁴⁴ Although such pressures have been preponderantly outward from the United States for many decades, there are, of course, counterpressures (for example, the recent heavy influx of Mexicans across our long Southwestern border) that strengthen the historic distinctiveness of some regions and serve as a counterforce to national cultural homogeneity.⁴⁵

⁴³ Theodore Brameld has ignored the *geography* of change but he has provided an interesting prospectus on the same phenomena; see Brameld, *The Remaking of a Culture: Life and Education in Puerto Rico* (New York, 1954). Studies by Daniel E. Doepfers deal with a fundamental aspect of culture during the Spanish and very early stages of the American imperial eras; see Doepfers, "The Evolution of the Geography of Religious Adherence in the Philippines before 1898," *JHG*, 2 (1976): 95–110, and "The Philippine Revolution and the Geography of Schism," *Geographical Review*, 66 (1976): 158–77.

⁴⁴ Ian Lumsden has provided a rather eclectic view of a range of mainly cultural topics; Lumsden, ed., *Close the 49th Parallel, etc.: The Americanization of Canada* (Toronto, 1970). And Gerard F. Rutan has usefully reminded us that the pressures are not entirely unidirectional; Rutan, "The Ugly Canadian: Canadian Purchase and Ownership of Land in Whatcom County, Washington," *Social Science Journal*, 14 (1977): 5–16.

⁴⁵ For an excellent illustration of the fact that imperialism on any major scale may continue to alter the internal geography of the imperial state long after the formal geopolitical relationship has ended, see James P. Allen, "Recent Immigration from the Philippines and Filipino Communities in the United States," *Geographical Review*, 67 (1977): 195–208.

The powerful and uneven spread of American influence well beyond our political borders is a major historical geographic phenomenon. Such a peripheral pattern represents the "sphere" in the geographical morphology of America as a macroculture; that realm of penetration and acculturation is typical of the outer, variegated edges of aggressive societies. Even a preliminary mapping of some broad categories of this outlying realm would begin to refine common understandings of America's global position and would help us see America as a culture area as well as a national state and a participant in world affairs.

THIS IS A ROUGH SKETCH OF SOME DISCERNIBLE CONTOURS OF AMERICA as seen from a rather different perspective. It is a view of America as a gigantic "geographic growth"—not as a quasi-natural phenomenon but simply as a cultural historical fact, as a continuously expansive pattern that can be traced on the surface of the globe from its tiny, variegated beginnings as tenuous transoceanic outposts of various European agencies to its solid transcontinental and oceanic national presence and multifarious penetrations of many other parts of the earth. It is a view that is at once fundamentally historical and geographical. It is a view of America as an ever-changing *place*, an ever-changing *congeries of places*, an ever-changing *structure of places*, and an ever-changing *system of places*. Those several views of place are held together by regarding places as the creations of particular peoples working over a period of time in particular locations and physical environments that are thereby stamped with a distinctive landscape and social character and organized as segments of spatial systems, all of which can be examined consistently at scales ranging from the local to the global.⁴⁶

Because this approach gives major emphasis to understanding and assessing the significance of the areal structure of the developing nation as it is altered era by era (these eras should become defined in terms of such basic geographic changes), it may become labeled as a regional approach, but it is critical to distinguish it from some all-too-common concepts of regions and regionalism. The regional concept has been too readily dismissed as a crude tool, whereas it is really a basic tool that has been used crudely. It is, in fact, a set of tools. The approach suggested here is a far cry from the physiographic frameworks of early geographers, the static subregions and blocks of states of the Odum School of regional sociology, or the broad sections of Turnerian historians.⁴⁷ It is an approach that sees regions as complex, ambiguous, and changing phenomena. Regions are abstractions, they exist in our minds. As a form of territoriality they can become imbued with emotion and can influence our actions, but we are using them here first of all as tools of thought, as means of analysis and synthesis. Such regions are not, therefore, units and

⁴⁶ And, thus, geography defined in terms of a focus on places ranges through the whole spectrum from idiographic depiction to theoretical abstraction, from esthetic appreciation to scientific explanation.

⁴⁷ I do not, however, mean to denigrate the seminal geographical work of such people; Turner was well ahead of the geographers in many respects. See Donald G. Holtgrieve, "Frederick Jackson Turner as a Regionalist," *Professional Geographer*, 26 (1974): 159–65.

they do not change as units but only through changes produced in their constituent elements. Thus, analytical use of a regional approach requires a geographic study of these elements and of subcomplexes such as cultural landscapes, social geographies, and spatial systems. These three subcomplexes provide a focus on a set of interrelated but rarely concordant areal patterns and will, therefore, always display the complexity, problems, and hazards of regional generalization.

The approach is more properly labeled geographic than regional, reserving the latter term to refer to a scale of areal generalization. There are affinities here with the thesis of David Russo's "new view of American history." He has stressed that "American society has been in the most basic way a *federal* entity—not just politically, but socially, economically, and culturally, as well," and he has called for a framing of historical inquiry so as to emphasize those levels of community that are most directly significant to the life of the people in each era.⁴⁸ He has criticized the imposition of a national perspective upon the whole course of American history and has, instead, insisted upon the historiographic significance of the obvious fact that, even after the nation has become a powerful force, people still live simultaneously in a locality, a state, a region, and a nation. This sense of areal hierarchy is fundamental and routine to the geographer's way of looking at people and places.

A corollary virtue inherent in the geographical perspective is its areal comprehensiveness: all places must be considered at all times. At any particular time, of course, some places will be more important than others, but this must be specified in terms of the entire areal surface and structure. Such an approach does not insure more accurate interpretations, but it does prohibit exclusion from consistent consideration any area—or people, because place as used here includes people defined in terms of culture and society. America may indeed be more of an extension from New York City than from Charleston; but since 1670 Charleston has existed and its position and significance must be routinely assessed through all eras. Such an approach would force us to recognize Sullivan's Island as a portal to America long antecedent to and in the long run only somewhat less significant than Ellis Island.⁴⁹

The basic concept of this approach, then, is simply that all areas and their peoples are of fundamental significance at all times to our understanding of America. The task—one of description, analysis, and assessment—leaves open the matter of interpretive themes. We begin, perforce, with some of these themes at least latently in mind, and they will be reshaped as we examine the evidence within the framework of our inquiry. Whatever these may be, we can be confident that a vigorous effort by geographers and historians to give real

⁴⁸ David J. Russo, *Families and Communities: A New View of American History* (Nashville, 1974), 2.

⁴⁹ I am obviously indebted for this illustration to Peter H. Wood's stimulating study: "Here was the thin neck in the hourglass of the Afro-American past, a place where individual grains from all along the West African coast had been funneled together, only to be fanned out across the American landscape with the passage of time. Sullivan's Island, the sandy spit on the northeast edge of Charlestown harbor where incoming slaves were briefly quarantined, might well be viewed at the Ellis Island of Black Americans"; Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), xiv.

substance to this sketch would result in a very considerable redefinition of "America."⁵⁰ To some degree this task will also result in a redefinition of the working relationships between geographers and historians in America. It shifts the balance of historical geographical inquiry away from revisionist history. Studies by geographers that subject standard historical interpretations to fresh scrutiny with new tools applied to old and new evidence stand as some of our very best work, admired by scholars in both fields,⁵¹ and more of that kind should obviously be welcomed. It is no denigration to assert that in such studies the geographer may be viewed as a servant in the field of history because it is the historians who have defined the problems.⁵² This prospectus implies a different relationship. To see America as a historical phenomenon in geographical terms is to produce a different set of problems for study, some almost entirely new and others so altered in context as to require extensive fresh inquiry. That alone does not make it better—only different. This geographical approach may prove to be a useful critique of some current views, but it should not be undertaken merely for that reason, but because it seems an inherently important perspective. What I have proposed is an addition to our ever-changing set of views rather than an alternative to any one of them.

AND SO WE MAY WELL END AS WE BEGAN—with attention to basic complementarities. Geography is a rather quieter field than history. It does not commonly focus very directly upon persons, actions, events, movements, and crises or even upon what may seem to be the dominant concerns of a people in a particular era. Yet in its focus upon places geography is ever involved in describing, analyzing, assessing, and interpreting qualities and changes related to a fundamental dimension of all life; and therein lies the basis for an enduring partnership with history. Twenty years ago the political geographer Stephen Jones offered a useful schematic illustration of the links between *idea* and *area* that are directly pertinent to the relationship between history and geography. *Ideas* begin in persons and are made effective through *decisions* that, in turn, generate *movements* (quite literally—setting in motion information, people, goods, money, etc.) that tend to create *fields* of activity that come into focus on specific *areas* in which some impact can be identified.⁵³ This

⁵⁰ Given the basic concept stated in the opening sentence of this paragraph, we can also connect this approach with that called for by Philip Wagner; see his "A Sense of History," *Proceedings of the Association of American Geographers*, 3 (1971): 178–81. Although this prospectus gives much attention to the regional and national scale, by attempting to build from local geographies, from the evidence of houses and farms and factories and local communities, there is at least a chance that "the counter-case to progress" will not get lost in the emphasis upon dominating institutions and systems.

⁵¹ A number of monographs deriving from dissertations under Andrew H. Clark at Wisconsin are outstanding examples. The selection of James T. Lemon's *The Best Poor Man's Country* (1972) for the Albert J. Beveridge Award by the American Historical Association is the most prominent recent recognition by historians.

⁵² Let it be clearly understood that any implications of relative prestige or power of the two fields are irrelevant here. I am concerned only with possible limitations on potentialities for the widest range of fruitful collaboration.

⁵³ Stephen B. Jones, "A Unified Field Theory of Political Geography," *AAAG*, 44 (1954): 111–23.

concatenation of *idea-decision-movement-field-area* can, of course, be studied from either end, but the tools needed and the results obtained are certain to be very different. If our primary interest is in the making of decisions and in the events emanating therefrom, we might best look to the historian. If we are primarily interested in areas and the kinds of changes they undergo and something of how these were effected, we might best turn to the geographer. If we wish to foster more fruitful interaction between the two, we might best focus more explicitly upon realms of *movement* and *field*.

Geography and history are bound together by the very nature of things: history takes place, and places are created by history. In America the immense possibilities for building upon that fundamental fact remain largely unrealized. We have not yet even reached a common understanding that the role of geography is not simply to provide the physical stage to keep history from "wandering as a vagrant without a certaine habitation."⁵⁴ As set forth in this prospectus, geography aspires to be nothing less than the whole drama on this earth viewed from a special perspective; geography, therefore, is not just the servant of history, it is something very like history.

⁵⁴ The phrase in quotation marks is a portion of the oft-cited, felicitous, but (in modern terms) misleading statement from the opening of the fifth book of John Smith's early history of colonial America: "Before we present you the matters of fact, it is fit to offer to your view the Stage whereon they were acted, for as Geography without History seemeth a carcase without motion, so History without Geography, wandreth as a Vagrant without a certaine habitation"; Smith, *The Generall Historie of Virginia, New England, and the Summer Isles* (London, 1624), bk. 5. It seems to remain insidiously congenial for historians to think of history and geography in terms of "man and the land" rather than in the terms used in the opening paragraph of this prospectus.

Comments:

PROFESSOR D. W. MEINIG AND I ARE WORLDS APART in our views on historical geography. His prospectus, urging thick description of spatial patterns and regional synthesis, fails to achieve the full potential of the discipline we share. Historical geography achieves greater potency, in my judgment, as an interpretive discipline committed to understanding the causes of significant variations from place to place in the economy, society, and polity. If we are to be taken seriously by historians, geographers must set aside the esoterica of vernacular architecture, town plans, and the like insofar as these are regarded as ends in themselves and turn to the fundamental concerns of history and social science. Recent research by historical geographers reveals that we are identifying decisive interpretive issues in American history and that historians are acknowledging our attention to these time-honored issues. What follows is a brief progress report on actual and potential geographic perspectives on the pre-1860 American economy, society, and polity.

THE SPECTACLE OF AMERICAN ECONOMIC DEVELOPMENT has eluded interpretation by a single discipline, and, accordingly, geographers have sought an understanding of urbanization's contribution to that process. Two issues have been paramount: regional divergence and selective urban growth within regions. Staples and their marketing seem to provide a key to explaining the abrupt contrasts between Northern and Southern urban systems. During the mercantile era (before 1800), a network of towns connected by linear routes to coastal ports conveyed varied American staples from the back country to consumers in Europe and the West Indies. The size of cities, however, varied sharply from North to South and was determined by staple bulk, weight, and perishability, and, hence marketing costs of the products they handled. Tobacco and, later, cotton were easily marketed and the earnings they contributed to the Southern mercantile sector were small; Northern grains, by contrast, commanded large mark-ups, which favored merchants and forwarders in growing urban centers. Regional urban contrasts hinged more on staple characteristics than on the slave or free labor forces in those regions.¹ Northern staples gave that region an initial advantage in urban development, and by 1790 that area was primed for what Allan R. Pred has labeled cumulative

¹ James E. Vance, Jr., *The Merchant's World: The Geography of Wholesaling* (Englewood Cliffs, N.J., 1970); Joseph A. Ernst and H. Roy Merrens, "'Camden's Turrets Pierce the Skies!': The Urban Process in the Southern Colonies," *William and Mary Quarterly*, 3d ser., 30 (1973): 549-79; and Carville Earle and Ronald Hoffman, "Staple Crops and Urban Development in the Eighteenth-Century South," *Perspectives in American History*, 10 (1976): 7-78.

growth. In the cities that later became Megalopolis, information circulated rapidly and efficiently, compounding the advantage they acquired during the colonial years. Owing to its centrality in an incipient Megalopolis, New York achieved especial importance.²

Historians and economists are taking these notions seriously, as is shown by Diane Lindstrom's recent critique of Pred's concept of the mercantile city and the role played by the flow of information in urban growth during the years 1790–1840. She attributes Philadelphia's growth in this period to a sectoral shift from mercantile activity to manufacturing.³ Although I am persuaded by her evidence of such a change, we must now explain the paradox of an agricultural recession accompanied by the introduction of factories and machine technology and by the rapid growth of towns. I am currently examining this change, and I say so only to make the more general point that geographers are becoming enmeshed in the crucial issues of urban and economic "take-off." Like it or not, geographers are inextricably involved in the debate on American economic development.

Geographers are also engaged in the hot debate over regional interdependence in the two decades before the Civil War. Sam Hilliard's highly respected monograph reveals the weak links connecting the West and South and shows widespread Southern self-sufficiency and independence of Western grains. The spatial separation of West and South is also demonstrated in Edward K. Muller's careful study of southwestern Ohio, where transport routes were increasingly aligned eastward and urban growth was biased at the resulting transport intersections.⁴

It should not be surprising that geographers attentive to the economy should also endorse rationality as a guiding principle for American behavior. The attempts of Kenneth A. Lockridge and James Henretta to cast the American farmer in the role of peasant or precapitalist have been skeptically received by geographers, who have been more persuaded by the farmer's extensive commitment to expanding markets.⁵ James T. Lemon has made capitalist rationality (liberalism in his words) the cornerstone of his interpretation of colonial southeastern Pennsylvania—economic considerations overrode the segregating influences of ethnicity and culture.⁶ But the point has stirred much debate. We may hope for a continuation of Lemon's work into the early nineteenth century, when the best poor man's country fell on hard times. After the War of 1812 poverty became widespread in city and

² Pred, *Urban Growth and the Circulation of Information, 1790–1840* (Cambridge, 1973), and *The Spatial Dynamics of U.S. Urban-Industrial Growth, 1800–1914: Interpretive and Theoretical Essays* (Cambridge, 1966).

³ Lindstrom, *Economic Development in the Philadelphia Region, 1800–1850* (New York, 1978).

⁴ Hilliard, *Hog Meat and Hoecake: Food Supply in the Old South, 1840–1860* (Carbondale, Ill., 1972); and Muller, "Selective Urban Growth in the Middle Ohio Valley, 1800–1860," *Geographical Review*, 66 (1976): 178–99, and "Regional Urbanization and the Selective Growth of Towns in North American Regions," *Journal of Historical Geography*, 3 (1977): 21–39.

⁵ Lockridge, *A New England Town, The First Hundred Years: Dedham, Massachusetts, 1636–1736* (New York, 1970); and Henretta, "Families and Farms: Mentalité in Pre-Industrial America," *William and Mary Quarterly*, 3d ser., 35 (1978): 3–32.

⁶ Lemon, *The Best Poor Man's Country: A Geographical Study of Early Southeastern Pennsylvania* (Baltimore, 1972).

country alike. Liberalism had failed for some, and that failure was acknowledged by such disparate groups as Hamiltonians, humanitarians, and union organizers. This fascinating period of poverty and spectacular urban growth assuredly deserves its historical geographer.

Amoral capitalism appears again as a theme in my study of slavery and free labor. American farmers chose slaves or free labor by rationally calculating costs and productivity applied to regional staples. The North rejected slavery because it was inefficient for wheat production; slaves became the rational economic choice, however, when the lower Midwest shifted to corn in the 1840s and 1850s. Apologias for and advocacy of slavery emanating from the corn belt justified Northern fears of an expanding slavocracy and made conflict between sections "irrepressible."⁷

Attention to antebellum economy and politics has not precluded geographic analysis of social structure, mobility, and ideology, though I sense that social historians have contributed a great deal more than geographers on these matters. Several geographers have clarified the process of frontier migration, capital accumulation, and social mobility, once again emphasizing the integrative influence of the economy over the isolating effects of culture.⁸ We are just beginning to understand the vast rural migration to urban centers that John Modell has skillfully documented.⁹ The uprooting of Northern farm labor owes a great deal to seasonal unemployment and small annual earnings, which provided a cheap labor pool easily induced to the city by merchants and manufacturers. Paradoxically, the rural South held on to its white laborers tenaciously, and the impressive rural opportunities for overseers and yeomen in the prosperous 1850s help explain why these laborers remained in the country.¹⁰ We are in need of a geographic assessment of the Southern yeomenry—an assessment that would resolve the controversy between Frank L. Owsley and Fabian Linden¹¹ and explain the yeomen's intense loyalty to the slave system that reputedly exploited them.

HISTORICAL GEOGRAPHY'S ACHIEVEMENTS over the past two decades are impressive; yet much remains to be done. Professor Meinig has already staked

⁷ Carville V. Earle, "A Staple Interpretation of Slavery and Free Labor," *Geographical Review*, 68 (1978): 52–65.

⁸ Robert D. Mitchell, *Commercialism and Frontier: Perspectives on the Early Shenandoah Valley* (Charlottesville, 1977); and John C. Hudson, "Migration to an American Frontier," *Annals of the Association of American Geographers*, 66 (1976): 242–65.

⁹ Modell, "The Peopling of a Working-Class Ward: Reading, Pennsylvania, 1850," *Journal of Social History*, 5 (1971): 71–95.

¹⁰ Carville Earle and Ronald Hoffman, "A Note on Transfer Wages and American Economic Development," paper delivered at the Meeting of the Association of American Geographers, New Orleans, Spring, 1978.

¹¹ This controversy has focused on the white, nonslaveholding planters, with Owsley suggesting considerable opportunity and social mobility for the "plain folk" of the Old South and Linden countering that this group was impoverished by competition with plantation slavery. For the most complete statements of their respective positions, see Owsley, *Plain Folk of the Old South* (Baton Rouge, 1949); and Linden, "Economic Democracy in the Slave South: An Appraisal of Some Recent Views," *Journal of Negro History*, 31 (1946): 140–89.

his claim for historical geography; so let me map out another territory. In a nutshell, the questions we address are time-honored historiographic issues, examined from either spatial or ecological perspectives. Our questions are nothing more than the central issues of social science and history—of economy, society, and politics. I await the geographer who examines and interprets the geography of the Great Awakening, of loyalism, patriotism, and disaffection during the Revolution, of union organization and working-class culture in northeastern cities, and of the Southern yeomen and Northern laborers.

Professor Meinig and I disagree on another score. He sees continuous imperial expansion as the grand theme of American historical geography; that theme, however, is subsumed by the broader one of capitalist ideology—a theme that integrates a vast range of American behavior. Capitalist ideology leaves room for marked discontinuity in American historical geography. Certain regions, jarred by capitalism's excesses, reacted to and altered that ideology at decisive moments in our past. Perhaps geographers should seek to understand those persons and regions that halted or slowed down the American juggernaut; I think of Philadelphia's Mathew Carey, Massachusetts' abolitionists, and Illinois' Abraham Lincoln. They and others managed to deflect our headlong course of using men as property, and in doing so they left a vastly different American society and landscape. I suspect that Professor Meinig believes that these questions lead us beyond geography, but, if that is so, then it is too late to stop the exodus.¹²

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¹² For an example of new approaches in historical geography, see the imaginative essay by David Ward, "The Victorian Slum: An Enduring Myth?" *Annals of the Association of American Geographers*, 66 (1976): 323–36.



DONALD W. MEINIG'S PAPER IS REFRESHING among calls to historians for interdisciplinary action, both for its substantive suggestions for historical research and for its willingness to recognize that interdisciplinary work involves the sharing of ideas and concerns on an equal basis rather than the importation of methods from a supposedly well-developed neighboring discipline into an allegedly underdeveloped history. Arguing that history and geography share a common interest in time and space, differing only in emphasis, and a common approach to understanding through synthesis and description, not through theory development and model building, he suggests that the disciplines ought to find ground for cooperation above all in rewriting American history. He therefore proposes what he modestly calls "a spare and tentative outline for an interpretative geographic study of our national experience."

By grouping topics for large-scale research in American history around geographic themes, Professor Meinig systematically demonstrates what the previous contributions of historical geographers have suggested for scattered fields: that an awareness of geographical dimensions of change and spatial patterns of interaction can contribute much to historical understanding. Particularly salient in the outline is a much more rigorous, geographical sense of region. Historians have traditionally used political units such as counties or states to define geographical limits of study instead of performing the kinds of detailed analyses of distributions of cultural traits, networks of economic interaction, and patterns of social identity that enable geographers to study regional variation more precisely. In my own specialty of early American history, for example, students of community have relied heavily on the basic units of local government for definition of their "communities" and so have found themselves restricted largely to New England, where the towns left good records of local intercourse, and have been constrained to understand community in a formal sense that assumed much about the actions of people under study. Similarly, but on a much larger scale, students of later America have defined studies of sectionalism largely in political terms, leaving less fully explored the geographical questions of distinctive cultural evolution and of the detailed description of the economic, social, and institutional patterns of movement and interaction that tied sections together. Professor Meinig's essay shows how much we can learn by building our geographic units out of a greater diversity of spatial patterns and by studying more consciously the spatial dynamics that separated or drew together the people of the past.

A key aspect of Professor Meinig's approach is his presentation of geography as a strategy for thinking about problems in spatial terms rather than as a body of theories and accompanying methods to be applied to specific problems. The outline is indeed constructed out of a series of large-scale, rudimentary theories, but these are presented as organizing devices and mechanisms for exposing specific research questions as much as elements for final explanation. For example, consideration of the industrial, cultural, and political domination of the Northeast during the last quarter of the nineteenth century and the first quarter of the twentieth, described in terms of a "quasi-imperial structure" of "core and domain," suggests a range of studies about the territorial organization of business in decisions in a host of industries and firms as well as about the location and spatial reach of a whole series of institutions concerned with information collection, processing, and dissemination. Both the larger categories of the outline and the specific topics for research aim at a form of understanding that is descriptive and largely nontechnical in method and predominantly humanistic in tone. Even those topics that are technical in nature seem to involve generally available skills, such as statistical manipulation, rather than proprietary methods that are a disciplinary monopoly. Because geography in these terms becomes a strategy rather than a single method, it contains a potential for presenting a different and enriching perspective on almost any topic of historical concern, and

Professor Meinig has succeeded admirably in showing how American history would be fuller for its use.

AS THE TENOR OF THIS DISCUSSION SHOULD INDICATE, I agree enthusiastically with the notion of mutually beneficial cooperation between history and geography, but I think some comments on the specific program may be in order. First, some emphasis on the place such a proposal occupies in the literature of geography seems appropriate. As Professor Meinig observes in a footnote, geographers have divided in recent decades over the nature and direction of their profession.¹ One group has followed a path of social scientific development and has attempted to elaborate formal theories and models of spatial behavior with an ultimate goal of developing geography as a theoretically and methodologically rigorous "spatial science." This scientific approach has involved all areas of geography, but it has developed most extensively in the subfield concerned with economic matters, where linkage with sophisticated economic theory has been possible. Economic geographers have created a series of elaborate theories, of which central place theory is the most widely known, to describe the spatial dynamics of economic activities. Meanwhile, a second major group of geographers, among whom historical geographers such as Meinig are prominent, have retained the more traditional methods of descriptive and synthetic analysis and have remained skeptical about the desirability and, even, the possibility of turning geography into a formal science. Although much of the philosophical literature has promoted the idea of a sharp cleavage between the two camps, the practical division has been less extreme, revealing a range from would-be scientists to what might be called "staunch traditionalists," with many geographers adopting the intermediate position of employing formal theory when it aids their substantive work but rejecting the more extreme scientific ideology. Many historical geographers, for example, use, criticize, and refine locational theory when studying economic topics even though they largely reject the "spatial science" position.

Within the spectrum of practical methods, the current prospectus occupies a predominantly traditionalist position. Questions of location are handled in a descriptive manner, and potential applications of spatial theory receive little emphasis. Such an approach is both helpful and legitimate: Professor Meinig has aptly observed that there is no single, best geographic framework. But I think that historians and geographers can profit from some of the more formal work in locational theory—and in other areas as well—without adopting uncritically every detail of a literature that has admitted deficiencies in explaining historical relationships. A framework established to exploit theoretical literatures would probably emphasize economic issues more strongly

¹ See the studies cited on pages 000–00, notes 1–2, above. For another helpful work, see Marvin Mikesell, "Tradition and Innovation in Cultural Geography," *Annals of the Association of American Geographers*, 68 (1978): 1–16.

than that under discussion, which is oriented toward what geographers call culture, but it would be nonetheless geographical. Moreover, there is ample scope for using formal theory within the current proposal.

A second observation involves the relationship between the research program suggested by Professor Meinig's prospectus and the geographical literature cited as illustrative and exemplary of work to be done. His decision to cite exclusively geographical sources serves the useful purpose of providing entry points for historians into the geographical literature and is thoroughly defensible on those terms. Under the circumstances no need exists for an argument that the historical literature contains numerous works that would directly contribute to a geographical history of the United States. We must, nevertheless, be as cautious about using the conclusions of existing geographical studies as we are about the spatially unsophisticated findings of historians.

The ahistorical propensities of some "spatial science" theories have received adequate attention,² but rather less scrutiny has been given to the special sense of time employed by much of cultural geography. As Professor Meinig observes in his introductory comments, historians share with geographers a common concern with space and time. In explaining the viewpoints of the professions, geographers often argue that historians are primarily concerned with change over time while geographers emphasize spatial process.³ Such an assertion underemphasizes the degree to which historians worry about past things synchronically, in time, as well as diachronically, across time. The difference of perspective is of practical importance, for example, in many cultural geographical studies, especially those that study the diffusion of cultural traits or objects in order to map out culture regions. Such studies usually focus on the location of surviving artifacts from several periods as a clue to changing diffusion patterns, and are less concerned with relationships among the surviving artifacts of the period, those artifacts that have disappeared, and other features of life at the time.⁴ Such studies seem to me to be seriously deficient for understanding past periods, although they may be very useful for tracing the origins of current regional patterns. (That some scholars try to minimize biases introduced by the destruction of artifacts by excluding subsequently urbanized areas from their studies merely reinforces the limitation on the historical usefulness of such analyses.) My point is not to denigrate geographical studies but merely to emphasize that they, like historical studies, will have to be recast in a truly geographic history.

Finally, let me recall Professor Meinig's proposal for a conceptual framework and his outline of major headings for study. Conceptualization can operate at several levels, and what seems especially valuable in this essay are

² For commentaries on central place theory, see, for example, the studies cited on page 1190, note 10, above.

³ See, for example, Cole Harris, "Theory and Synthesis in Historical Geography," *Canadian Geographer*, 15 (1971): 159.

⁴ This distinction seems to underlie the curious exchange between Wilbur Zelinsky and several historical geographers in 1973 and 1974; see Zelinsky, "In Pursuit of Historical Geography and Other Wild Geese," *Historical Geography Newsletter*, 3 (1973): 1-5; and the replies of John A. Jakle, Alan R. W. Baker, and D. W. Moodie, J. C. Lehr, and J. A. Alwin, *Historical Geography Newsletter*, 4 (1974).

the general proposals for studies that focus on regions, their subsidiary “cultural landscapes and social geographies and spatial systems” in the historical past, and the still more general view of the potential for harmonious cooperation. The specific categories of the outline must be seen in largely instrumental terms, as organizing headings that evoke a legion of spatial historical questions. That Professor Meinig has succeeded in suggesting a general framework for cooperation and in placing the call for specific research in a clearly subsidiary position testifies to the breadth and flexibility of his “Prospectus.”

EDWARD M. COOK, JR.
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Reply:

IT IS GRATIFYING TO HAVE TWO HISTORIANS—the editor of the *American Historical Review* and Edward M. Cook, Jr.—so generously welcome a geographer's rather audacious proposal for a different kind of history. That my prospectus has caused my fellow geographer, Carville Earle, to squirm with discomfort and extol another kind of collaboration may serve as a useful caution. Although I have grounded my view in the logic of our fields as I see them, the prospectus is idiosyncratic and is not offered as representative of the stance of any particular group or proportion of American geographers. It is, however, only fair to note that in keeping with the original invitation the first version of this paper,¹ which both of these respondents also read, was aimed somewhat more directly at general possibilities for relationships between the two fields. I accepted that assignment with misgivings, and I think Earle's alarm that historians might mistake my outline as representative is in part a reaction to that original paper. I am quite willing to accept the inference that I was not the most appropriate spokesman for that occasion.

I AM ESPECIALLY PLEASED to have Professor Cook accept my view of geography in its broadest terms as a “strategy for thinking” rather than as a body of specific theories and methods. And it is refreshing to be able to discuss such matters with someone who recognizes that much of the actual work of geographers (as of historians) lies intermediate within those oft-cited philosophical polarities of description and explanation, idiographic and nomothetic, art and science. I do indeed put great emphasis upon description because I regard it as both fundamental and satisfying. I view explanation and interpretation as essentially forms of description at a different level, which makes them efficient tools for thinking that may lead us to fresh understandings, at times opening up exhilarating new vistas, as the field of history richly attests. But history also attests to how myopic and distorting, how vulnerable and short lived, explanations and interpretations can be. They are always tentative, contingent upon partial evidence, idiosyncratic assessments, and the ways a particular generation views the nature of man and society. Thus, they are always being refined or replaced, they are fuel empowering the continuous reworking of history, whereas good description tends to endure. It has intrinsic value in defining qualities of the things described, and it persists as basic material with which to construct new explanations and interpretations. A little data on a major topic may be

¹ See the initial note, page 1186, above.

productive of exciting theories, and it is certainly true that it takes a special kind of mind to discern larger patterns within a mass of detail. But "mere description" is itself a basic stage of such discernment, and surely the "thicker" the description the richer the potentialities for more satisfactory and enduring theories.

Of course, one cannot describe effectively without having some sort of theory at least implicitly in mind. My prospectus is framed upon and laced through with theories, mostly elementary perhaps, but it lies open to the application of whatever proves useful in illuminating the content, structure, and functioning of the areas under study. Professor Cook is right in suggesting that many historical geographers would put more emphasis upon economic issues. I am conscious of my bias toward cultural rather than economic interpretations, and, in a somewhat fuller exposition of this outline that I hope to prepare, I shall make this clear and identify the basic concepts and theories that inform my work. But this prospectus does not specify an exclusively cultural emphasis; the whole course of the geographic growth of America that I have sketched, could also be interpreted primarily in economic terms. The results would certainly be different from and in some degree competitive with those I might produce, but, because I do not believe we can ever *explain* in any close scientific sense the development of a nation, culture, or society, they would also be in part complementary—illuminations of different facets of a thing so large and complex we can never bring it into full and clear focus. Thus, the more views the better. Professor Earle states that, "like it or not, geographers are inextricably involved in the debate on American economic development." Of course they are, but what a curious way of putting it—as if I objected to the fact.

PROFESSOR EARLE WORKS HARD TO DISASSOCIATE HIMSELF from my kind of historical geography. His critique, therefore, takes the form of urging an alternative approach. I have no objection to the topics he recommends. I certainly recognize that geographers, and he among them, have made important contributions to the study of "time-honored historiographic issues," but I cannot let him define the differences between us quite in his terms. For one thing, I cannot let him get away scot-free with his invidious distinction between those geographers who study "the esoterica of vernacular architecture, town plans, and the like . . . as ends in themselves" and those who, like himself, deal with "the fundamental concerns of history and social science." "Esoterica"? If the vernacular is esoteric, then we have neglected to pay attention to most of the people who make up the society we are ostensibly studying. Vernacular architecture is to landscape study what common men or ordinary persons are to social history. "As ends in themselves"? Quite the contrary, the geographers whom I cite have studied such things as being peculiarly revealing about values and beliefs, about conservatism and innovation, about relationships between technology and society, about the

extent, contacts, mixtures, and migrations of particular groups, about variations of culture in space and time—in short, “common houses, cultural spoor,” to use Peirce Lewis’s fine phrase. If such matters are not “fundamental concerns,” history and social science are ignoring much of life. Of course, they are concerned with such topics but both have tended to ignore valuable evidence of the kind Earle denigrates as “esoterica.”

Professor Earle is interested in “crucial issues” and “hot debates.” Fine. I am simply suggesting that geography also provides a strategy for thinking about the whole course of American development in terms quite apart from what are currently considered the “decisive interpretive issues.” Our strategies are certainly different: he begins with issues, I begin with areas. He joins the historians on their own terms, to explore the issues they have declared to be important; I hold to the geographers’ concern for places and invite historians to join in reviewing our history from such a stance, a fresh look that is certain to open up a whole new set of issues for debate.

I have no interest in arguing for the superiority of my approach over his. I only want to stress the great and fundamental differences in objectives and results. Consider one of his own papers, which he uses as an example: he demonstrates to his own satisfaction that “American farmers chose slaves or free labor by rationally calculating costs and productivity applied to regional staples.” I readily acknowledge that he thereby made a contribution to a major debate, but I find the debate itself quite peripheral to what I want to know about the United States in the 1850s. The specific motivation of behavior, whether it is “amoral capitalism” or something else, is not critical to the kind of geographical understanding I seek. To say so is not to deny the link between areas and ideas. But for my purposes the search for cause is not always primary and is never sufficient. I am interested not just in why men behaved as they did but in how they behaved and in how they created and altered the places with which they were involved. I am interested in what areas were like in times past, in the specific character of their cultural landscapes, social geographies, and spatial systems. Furthermore, as I emphasized in the prospectus, we must have such information comprehensively, for all places at all times, insofar as this is feasible.² No one is likely to produce such descriptions if we start with capitalist ideology as a major theme, or if we begin in the thickets of the decision-making process. I heartily applaud the proposal that we connect Lincoln to the landscape; I only insist that we will never get the kind of landscape description needed for the kind of reassessment I propose by focusing on Lincoln.

AGAIN I END ON THE THEME of the analogical character of our two disciplines. Such differences in the kinds of knowledge we seek constitute a persistent and

² And, for a pertinent illustration of this point, see Edward Cook’s own recent study of New England town government, in which he has noted the very serious limitations of our dependence upon the analysis of a few towns and the need for comprehensive coverage. His impressive efforts toward a complete areal survey, his typology and use of central place concepts, are a fine example of a convergence in some basic approaches of our disciplines. Edward M. Cook, Jr., *The Fathers of the Towns* (Baltimore, 1976).

genuinely creative tension within geography as within history. Individual positions on such matters are, I presume, a matter of temperament and training, an expression of what we find most satisfying and what we have been taught as most important. The balance within our fields is always delicate and always shifting. The healthiest condition for the field of geography (and, I assume, for the field of history) is the cultivation of a vigorous diversity enhancing the cross-fertilization of art and science; the most dangerous threat comes from monoculturists who would upset the balance of old and essential complementarities. Professor Earle's specific proposals seem good in themselves, but the stridency of his tone reminds me just enough of the more rabid "scientific geographers" to compel me once again to warn of the persisting, insidious efforts to reduce geography to mere explanation.

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Reviews of Books

GENERAL

REX MARTIN. *Historical Explanation: Re-enactment and Practical Inference*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 267. \$15.00.

Current theorizing about history by analytic philosophers still tends to take as its point of departure two enormously influential pieces of writing that appeared in the 1940s: C. G. Hempel's incisive article, "The Function of General Laws in History" (*Journal of Philosophy*, 39 [1942]: 35-48), and R. G. Collingwood's flawed but eloquent classic, *The Idea of History* (1946). The mutual confrontation of the positions paradigmatically expressed in these two works has generated a lively, interlocking, and increasingly technical literature. One of the merits of Rex Martin's book is that it provides guidance through the maze of philosophical argument and counterargument while seeking in some measure to provide a "via media" between the so-called positivist and idealist views of history.

In spite of disclaimers, the work does often seem to take for granted—a bit disconcertingly—that the concept of historical explanation is and ought to be the central problem of historical theory and that this problem is primarily one of determining how individual human actions should be understood. This somewhat myopic standpoint is surely what leads the author to observe that, although history is "the least developed of the social sciences" (a judgment for which no reasons are given), in the hands of analytic philosophers the philosophy of history has, surprisingly, become "more sophisticated" than the philosophy of other branches of social inquiry (p. 15). This alleged sophistication has largely been borrowed from theories of explanation in general or from what, in the analytic tradition, has sometimes been called "action theory"; and it has often coexisted with a rather low level of interest in the broad range of historians' preoccupations. (For a recent work in this tradition about which this can *not* be said, see Maurice

Mandelbaum, *The Anatomy of Historical Knowledge* [1977].) Still, the question of how past actions are to be explained, if somewhat limited, is one that historians with an interest in the conceptual foundations of their discipline can hardly afford to ignore. This is especially true of those historians who, Martin insists, practically "glow" at the mention of Collingwood's name.

What especially concerns Martin is explanation of actions by reference to the motivations, beliefs, intentions, purposes, or, to use Collingwoodian language, to the "thoughts" of those who performed them. His central question is the nature of the "tie" that must bind such explanatory data to the deeds they are said to explain. The Hempelian answer to this question is well known. According to Hempel, the nature of the "tie" is determined by the logical form of explanation as such. It consists of universal empirical laws which show the action performed to have occurred necessarily, given antecedent conditions which may include the agent's "thoughts." Hempel conceded that, in actual practice, all that historians may be able to do is show the probability of what was done, in the light of generalizations that are less than universal. But he regarded such an alternative as explanatory only to the extent to which it approximated the more rigorous ideal—that is, the probability would have to be high.

Three decades of philosophical bombardment have produced many ingenious variations on this basic position. Most of them endeavor to retain some important residue of the claim that the force of an explanation derives ultimately from its connective empirical laws while trying to do justice to apparently quite respectable historical practice that suggests the contrary. Martin considers and rejects a number of these variations, including perhaps the most ingenious of all—Morton White's contention in *Foundations of Historical Knowledge* (1965), that, although an explanation can be no

better than the laws which it states or assumes, historians may justifiably offer explanations in ignorance of the latter provided they have good reasons for thinking that appropriate laws exist. Martin finds this "existential regularism" unsatisfactory on what would appear to be the eminently reasonable ground that one cannot know that a law "exists" without being able to establish it. And he concludes, in the end, that the whole enterprise of searching for the force of action explanations in empirical laws involves a conceptual error of a fundamental kind.

A better guide to the nature of such explanations, he suggests, is to be found in Collingwood's controversial notion that historians must re-enact the thought of the agents whose actions they wish to understand; and he devotes the early chapters of his book to an "archeological reconstruction" of this claim. He takes issue with the view popularized by W. H. Walsh that it was Collingwood's intention in this connection to promote an intuitive theory of historical understanding. He denies, further, that the re-enactment requirement is to be interpreted primarily as a point of historical methodology: it is a claim, he argues, about what the understanding of actions consists of, not about how such understanding, otherwise defined, is to be achieved.

The problem is how one is to contrast the re-enactment doctrine, so interpreted, with an analysis like that of Hempel. Collingwood flatly denied that laws play any necessary role in the understanding of actions via re-enactment—in spite of allowing, as is not always noticed by those who comment on his views, that historians may often be able to formulate generalizations which hold good at least for certain times and places. But he never worked out the logical implications of his denial; he never made clear the nature of the "tie" that, in the absence of laws, makes certain "thoughts" explanatory and others not. Martin notes an attempt some years ago by the present reviewer to work out an essentially Collingwoodian position in this connection—one in which "thoughts" were said to be explanatory when related to actions by rational principles rather than by empirical laws. This quasi-normative account (*Laws and Explanations in History* [1957]) he commends for seeing action explanations, not as weak law subsumptions, but as attempts at an understanding of an entirely different kind. But he finds it ultimately unsatisfactory, chiefly because what a principle of action would explain is not why an action was actually performed but only why it was rational for the agent to perform it. Hempel similarly argued that such a principle would be explanatory only if reinforced by the assumption that

the agent belonged to a class of agents who in fact act in accordance with that principle so that the explanation would be grounded, after all, in empirical generalization.

Most of Martin's book is devoted to an examination of a quite different interpretation of Collingwood's re-enactment doctrine—one proposed first by Alan Donagan (*The Later Philosophy of R. G. Collingwood* [1962]) and developed further by G. H. von Wright (*Explanation and Understanding* [1971]) and a number of other authors. Referred to in recent analytic philosophy of action as "the logical connection argument," this conception of the nature of action explanation is too complicated to be set forth in its fully elaborated form in a review. (For an example, see F. Stoutland, "The Logical Connection Argument," in N. Rescher, editor, *Studies in the Theory of Knowledge* [1970], pp. 117–29.) But the main principle on which it turns is not hard to state: the explanatory relation, or "tie," between what an agent "thinks" and what he does takes the form neither of an empirical regularity nor of a normative principle, but of a logical truism or analytic judgment; the agent's acting as he does thus is logically entailed by his thinking the "thoughts" attributed to him. To illustrate this claim as simply as possible: if a person intends to achieve a certain goal and believes that a certain action is required for the achievement of that goal, then it is a matter of sheer logic, a matter of the very meaning of the terms employed (so it is argued), that he perform the action in question. For if he does not, that in itself would show that he lacks (or has lost) either the intention or the belief; to say otherwise involves a contradiction.

Actually, this schema needs some refinement before it can be said to be entirely true. It would have to be understood, for example, that the person concerned was not prevented from acting in accordance with his alleged intention and belief, that he possessed the ability so to act, that he was aware of the nature of what he was doing, and so on. But those who adopt the approach of the logical connection argument contend that such refinements could be fully stated without the reintroduction of either connective empirical laws or rational principles of action. Given an analysis of this sort, Donagan has interpreted Collingwood as implying (he admittedly does not quite say this) that historians explain actions by formulating hypotheses regarding agents' "thoughts" from which they can deduce, logically, what the agents in question would do. These hypotheses are then accepted as true and explanatory to the extent to which the agents act accordingly.

Martin offers two sorts of objections against the claim that the "tie" in action explanations is

analytic. On the one hand he argues—although rather too casually in the opinion of this reviewer—that, for the vast majority of such explanations (he seems willing to make exceptions for unusually firm intentions, like the resolve of Brutus to protect the Roman Republic “at all costs”) it does not follow strictly from the meaning of the concepts employed that the agent acted in the way he did. Martin does not, however, entirely reject the thesis that the connection involved is conceptual in nature. For while it is not, according to him, impossible in logic that a person should fail to act in accordance with his own intentions and beliefs (he speculates in this connection on what might conceivably have happened when Booth pointed his gun at Lincoln), it is nevertheless in some sense an *a priori* truth, and not just a factual discovery, that he will act for the most part in accordance with them.

Martin is not easy to follow when he tries to explain in exactly what sense the connection in such cases should be considered *a priori*. He sees some likeness between the basic schema for action explanations and the *a priori* principle that every event has a cause, the latter often being said to be neither logically necessary (it does not just follow from the meaning of the word “cause”) nor empirically refutable (when we fail to find causes we generally blame ourselves, not the phenomena). But he is not prepared to call it a “synthetic” *a priori* principle in the sense in which Kant, for example, regarded the causal principle as one; for Martin cannot see that it is a principle of human reason as such: it might lack validity for some societies, he thinks, although these would have to be different from our own in ways that are almost unimaginable. He takes refuge finally in Wittgenstein’s notion of a “language-game,” holding that something like the indicated schema is constitutive of our whole enterprise of interpreting our experience in terms of human agency. Historians will perhaps be content to let philosophers fight it out as to whether the “tie” in action explanations is *a priori* in a Kantian or a Wittgensteinian sense. The important thing is that the connection is alleged to be, in some important sense, conceptual rather than empirical or normative. For if true, this makes the explanation of past human actions, as Collingwood himself had insisted, a very different sort of thing from the scientific explanation of events—a logically different sort of thing.

Martin’s other objection to the analytic interpretation is also somewhat elusive. A candidate action explanation must not only conform to the general schema sketched above but also, of course, cite “thoughts” which are attributed to the agent on adequate empirical grounds—that is, historians must have reasons for thinking that their ex-

planatory premises are true. But Martin insists that this is not in itself enough. According to him, there is in addition the requirement that an instantiation of the schema must be shown to be empirically “plausible”; and this plausibility, he argues, restores a vestige of the original Hempelian position. Hence, he can claim to have found a “via media.” For to show empirical plausibility involves showing that the “thoughts” in question were at least “causally relevant” to the action performed; and this cannot be done without reference to at least weak empirical generalizations. Martin employs considerable technical apparatus to elucidate the notion of causal relevance; but the essential point seems to be that such relevance cannot be established without showing that people really do (occasionally) act in the way envisaged for the reason cited.

Martin’s own plausibility seems to evaporate at this point, along with his “Collingwoodianism.” For the suggested criterion is surely at once too strong and too weak. It is too strong because it excludes the possibility that a historian might correctly judge an agent to have acted in a certain way for a certain reason for the very first time, which it is difficult to believe he could not do. And it is too weak because, if an action explanation required the support of other cases at all, *some* other cases would surely be insufficient. We should want, with Hempel, enough of them to render the action in question probable, a requirement that Martin goes out of his way to repudiate. Furthermore, in the very statement of his additional criterion, Martin seems to fall into another difficulty. What we are told is that, for plausible explanation, we need to know that others have in fact acted for the reasons attributed in the present case. But if we can know that in other cases people have acted *for* those reasons (presumably not because of our knowledge of still other cases), why cannot we know this independently in the case under examination?

What is missing is any adequate account of what it is to act for a reason at all, rather than simply to have one and to act in accordance with it. It is discouraging that, after all the philosophical activity (and all the “sophistication”) of recent years, this notion, so crucial for any attempt to provide a rationale for history as a humanistic discipline, and perhaps for the foundations of the social studies generally, remains so murky. This complaint must not, however, be allowed to obscure the very real contribution made by Martin’s book both to the ongoing discussion of historical inquiry in general and to the interpretation of Collingwood in particular. His argument is richly textured, ranging over many important issues that could not be considered here: for example, the relativity of ex-

planation to description, the difference between explaining and understanding, the Collingwoodian notion of history as "the science of human nature," and Collingwood's "transhistorical" commitments.

In discussing these and other questions, Martin exploits, with generous acknowledgment, some of the best work done in the field, notably that of A. C. Danto, whose *Analytical Philosophy of History* (1965) is perhaps the most original of the current spate of monographs on history by analytic philosophers, and that of W. H. Walsh, whose influence has been continuous and salutary ever since the appearance of his *Introduction to Philosophy of History* (1951), still the best entree to the subject. Martin is not always easy to read: his style rather awkwardly combines the technical and the conversational. But he will be read—and, doubtless, answered.

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LAWRENCE STONE. *The Family, Sex and Marriage in England, 1500–1800*. New York: Harper and Row, 1977. Pp. xxxi, 800. \$30.00.

In spite of its monumental dimensions, this beautiful book by Lawrence Stone is delightful to read, thanks to the flavor of the examples he cites and the sly irony of his commentary (an irony that has also informed his excellent choice of illustrations). The logic of composition and the clarity of his writing are two qualities which have become so rare in historical literature that one cannot fail to applaud them! On the one hand, the book offers access to primary sources: a rich anthology of animated texts and biographical data, which the reader can exploit as he pleases. On the other hand, Stone offers a personal interpretation of the history of the family over a period of three or four hundred years, from the fifteenth to the twentieth centuries. The central part of the argument bears on the period from the beginning of the sixteenth century to the beginning of the nineteenth.

Clearly Stone preferred to limit himself to England with only a few very brief incursions into New England, and, alas, equally few, among the more "backward" peoples of Continental Europe. Nevertheless, in spite of a determined belief in British particularism, Stone's study is less "provincial" than he intended; it applies to the whole of north-western Europe, whose deeper cultural unity is played out against a background of various languages, political systems, religious faiths, and national styles.

The periodization employed by Stone comprises five main family types, which appear con-

secutively, but which also overlap or diverge depending on the social classes involved: 1.) the open lineage or traditional family (sixteenth century); 2.) the restricted patriarchal nuclear family (end of the sixteenth and first two-thirds of the seventeenth century), which I would call the repressive patriarchal family, type I; 3.) the closed domesticated nuclear family (late seventeenth and eighteenth centuries), which I would call the affective-individualist-permissive family, type I; 4.) the Victorian family, or repressive patriarchal family, type II; 5.) the family typical of the mid-twentieth century, or the affective-individualist-permissive family, type II. Types four and five are characterized by a relative return to types two and three.

Five different types in roughly five centuries amount to a lot of change! Social phenomena such as family, love, and death are located, I believe, at a level very near the biological basis of our being, at the boundary between the biological and the mental. Only recently have we begun to understand that they change, but change very slowly, so slowly that their transformations have until now eluded the perception of contemporaries. The very fact that these phenomena remain at the level of the unconscious add to their inertia. That is why one is surprised by the abundance of models in this book. This atomization by Stone of a massive phenomenon can be explained, I think, by two methodological considerations.

First, Stone situates attitudes toward the family, love, and sex in the same higher mental categories as religious *doctrines*, for example, Puritanism, evangelism, or deism, and political and philosophical *ideas*, that is to say, the more conscious and therefore the more malleable and *mobile* aspects of our total behavior. I think, on the contrary, that family, love, and sex belong to another level, where the psychological can hardly be separated from the biological. Between these two levels, interactions are rare, and each evolves at an independent pace without much interference from the other. Stone readily acknowledges this autonomy but does not draw any radical conclusions for his chronological scheme. Thus he finds as many changes in the family as in the higher religious culture.

Second, Stone has been influenced by, or rather *against*, the social sciences and their innumerable theories on the family. In general, these always tend to oppose a traditional family to a modern family, while invoking various explanations for the contrast. Stone has rejected, and I think rightly, this dualist simplification which does not take into account the diversity of social reality. In reaction, however, he has become involved in pulverizing the sociological models and exaggerating the factors of diversity, over both time and space, so that he obliterates a fundamental distinction, one

which may have been oversimplified by the sociologists and which we should probably adapt more sensitively and flexibly to historical reality, but a distinction that we should respect nonetheless. In fact, and in spite of himself, Stone has respected this distinction, as his title indicates. Most of the book deals only with models one, two, and three and he has added models four and five (dealing with the nineteenth and twentieth centuries) for the requirements of his thesis, that is, in order to reinforce his theory of the multiple causation of social phenomena and to counteract absolutely any dualist or linear theory. We can concentrate our attention on the first three models without betraying the underlying conception of the author—although that conception is sometimes camouflaged by the strategies of the argument he employs.

Now the problem presents itself in more familiar and general terms. We start from the end of the fifteenth century with the traditional, premodern family, which Stone has analyzed admirably. He insists on what he calls the “porosity” or “permeability” of the group formed by a husband and wife and their children within a more extended and constraining environment, that of kin and community. The socialization of young people occurred outside of their own family, in the families of others, in peer groups, and in general in the outside world. The real constraining factors were those of lineage, community, and patron/client or master/servant relations.

I will limit myself to only minor reservations concerning this remarkable portrait. The first deals with individualism. Scholars usually counterpose the individual to the collective forces in discussing traditional society, and so does Stone. But in premodern Western societies these collective forces did not intervene as constantly, or with such continuous impact, as they do in industrial society today. They were manifested sometimes through the divine aura with which individuals or institutions were invested, as in the case of the *Roi thaumaturge*, healer of scrofula, and sometimes through sudden, irregular interventions—a kind of theophany. And in between these manifestations there were empty periods, during which each individual—actually each male—beginning at an early age could manage his own domain, perhaps resisting community pressures or taking advantage of their relaxation, and calling upon all of his resources, including those of his kin and his wife, and upon their combined ingenuity. Only much later did modern feeling toward the family and the constant pressure of the contemporary state apparatus overcome the stubborn individualism which originated in the later Middle Ages.

My second reservation concerns “affectivity.” I

share Stone’s view that affectivity was diffuse and even precarious in this period, and that it was very much subject to the vagaries of the individual’s movements within his community or even outside it. But I do not believe affectivity was at such a level as he indicates. Nowadays, in our era of absolute *privacy*, we cannot easily comprehend the joy of being part of a group—a joy which could, of course, quickly turn to anger. Dostoevsky captured the ambivalence of this kind of sensibility very well. Neither do we readily understand the function of codes of polite behavior. Such rules now seem contrary to spontaneity of expression, whereas in the past these very rules were quite helpful in conveying feeling, albeit differently, since they imposed a certain discretion, made necessary by the unrelieved promiscuity of daily life (as was the case in Japan).

Stone’s treatment of the third model, which appeared in England around the end of the seventeenth century, is so thoroughly supported by the weight of consensus that there is not a great deal to add about it. He underlines very well the basic role of two factors: 1.) the triumph of affectivity (which he associates, wrongly in my opinion, with individualism) and 2.) the decline of kin and community. The so-called nuclear family stands alone in the social space thus cleared for it, left to its privacy. (I particularly liked Stone’s clarification of the debate which has recently agitated historians about the nuclear family versus the stem family.) All the new and expanded affectivity is from now on exclusively concentrated inside this closed family, expressed either between spouses or between parents and children. Romantic love (or the semblance of it) is now obligatory in marriage, where it had earlier been excluded. The family is child-oriented and from this point it tends to limit fertility: “Contraception is therefore only likely to happen in a child-oriented society in which bringing up the child and launching him into the world is becoming so burdensome in its demand for *love, time, effort, and money* [my emphasis] that some reduction in numbers is highly desirable” (p. 418). But for me, the nineteenth-century family is of the very same type; the austerities of Victorian morality do not change any of its fundamental characteristics.

We have before us, then, two types of family: a *terminus a quo* and a *terminus ad quem*. Everyone agrees more or less on the termini; but how do we get from one to the other? In this transition lies the entire difficulty. In order to explain it, Stone has devised his model number two: a patriarchal family typical of the late sixteenth and seventeenth centuries. This family type already tends toward the nuclear model because of the weakening of kin and community ties, but it differs from the closed

domesticated nuclear family insofar as the repressive authority of the paternal head of the family is more effective.

Stone finds that the ideology of the end of the sixteenth and beginning of the seventeenth centuries is dominated by the familial model. The king is *parens patriae*, as the father is the "king" of the family. Society is conceived of as a pyramid of families where the origin of authority is always paternal. This is indisputable and as true of the France of Bodin as it is of the England of James I. In France this sixteenth- and seventeenth-century family ideology continued to nourish the family ethics of the Catholic and counterrevolutionary Right in the nineteenth century, from Bonald and J. de Maistre to F. Le Play. The importance of this current in the history of ideas and also in the history of law cannot be denied. Stone thinks that it also influenced daily life—among the upper middle class of the towns and countryside (the squirearchy)—and that it reinforced the patriarchal powers to the point of creating a new type of family distinct from its predecessors and successors. I am quite convinced of the reality of some transitional types, but I do not believe that "patriarchy" is its specific nature. The doctrine of the family as the cell of society, and of the necessity of preserving and fortifying it, remained irrelevant to everyday concerns, except where such doctrines inspired legislative restriction of the civil rights of women, or those of marriageable youths.

During this period in which traditional sociability tends to disappear (but not totally) and the new affectivity takes effect (slowly before the 1660s), the family is characterized, for me, by an increasing interest among the elite in childhood education. Education is already recognized as an investment.

Stone insists upon the repressive traits which he attributes to the reinforcement of paternal authority: "the great age of the whip." He occasionally admits, however, that this severity of training has another meaning: "Paradoxically enough [but not perhaps so paradoxically!] this was the first result of a great interest in children" (p. 174). This quotation is of crucial importance, but Stone does not extract the most from it; he stops short. Yet he is one of the leading specialists in the educational history of this period (see "The Educational Revolution in England, 1480–1660," *Past and Present* [1964]), and he might have pursued the urge to establish a relationship between the extraordinary development of schools and colleges since the sixteenth century in England, France, and Spain (to mention only the countries where it has been most studied), and the transformations of the family. His silence on that subject in this book is surely deliberate. The reason probably being that, for

Stone, as for most sociologists, the school developed in opposition to the family and not through and for the family. The school, it is argued, allowed for the transfer of the socialization of the child from the family (the porous, permeable family of model one?) to the state, the Church, and impersonal, public institutions. The privatization of the family according to this hypothesis would arise from an amputation of the family, from the surrender of its educative functions to the state and to collective public institutions.

I believe, on the contrary, that the rise of the school is due more to the increased interest taken by families in their children's education and that it is this interest which sustained ecclesiastical, municipal, or other initiatives. That is why the distinctive characteristics of the family (and perhaps of the whole society) of the sixteenth and seventeenth centuries (before 1660) seem to me to be: 1.) the persistence of medieval emotive impulses; a massive resistance (and not only in the subordinate classes) to the combined moralizing pressures of Church and state powers; 2.) an "educational Revolution," to use Stone's excellent terminology, which caused a new (and modern) sensibility to flower at the very heart of a society which was, in many respects, still medieval, and which produced a new image of the child, and eventually, of the adolescent; 3.) consequent to these two phenomena, a family not yet fully freed from its collective environment (community, patronage system, etc.), but in which the newly acknowledged importance of the *cultural formation* of the child (as opposed to his *material well-being*) began to create a new feeling, a stronger, more affectionate one, between parents and children.

Finally, I would like to come back to a constant preoccupation of Stone, the particularity of Britain. I have not always followed his lead on this issue, but his insistence has compelled me to reflection. One point seems worthy of attention, although he has not raised it. Is there not a relationship between what he says about the low degree of affectivity in sixteenth- and seventeenth-century England, and the disappearance of the "thou" form from the language? This is really an extraordinary phenomenon, which, like any fact of language, takes place at the same deep level as non-conscious behavior, the level where I would also situate attitudes toward the family and the elementary forms of sociability. To my knowledge there are few languages where usage has so radically imposed the second person of the plural, thus increasing the distance between interlocutors.

In sum, one may ask if, at a later period, this disappearance from the language of the familiar form of address did not accelerate and reinforce the modern tendency to *privacy*, a term which has

no exact equivalent in French. Certainly France was also affected by the rise of privacy, and earlier than Stone would have it. But neither there nor in any other country in Continental Europe did privacy abolish other premodern forms of sociability as it did in England and in countries influenced by British culture. This psychological situation was not without consequences for the transformation of the city and for the urban culture of the nineteenth and twentieth centuries, with all the social consequences therein implied.

One can see just how far, step by step, family history can carry the reader, and how it leads to far-reaching speculations. The same would apply to the history of sexuality, which Stone has separated from his analysis of family models and condensed into the three chapters of the fifth and last section of his book. It is a passionate and original essay, loaded with examples, which itself calls for a commentary at least as extensive as the present one, and which it would be impossible to summarize in a few sentences. We must be content to call attention to it, and to beg the reader to pardon the reviewer if he has failed to exhaust in a few pages the richness of this important book.

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PHILIPPE ARIÈS. *L'homme devant la mort*. (Univers Historique.) Paris: Éditions du Seuil. 1977. Pp. 641.

This is an encyclopedia of death and dying, of attitudes to death and to the dead, from the beginning of Christendom to the twentieth century. Those who want to know Ariès's essential thesis can read the essays in his *Western Attitudes Toward Death* (1974). Those who have time for more should read—I almost said for fun!—this more leisurely compilation, meandering like a deliberate stream through varied countryside, the course vastly more important than its conclusion, the weeping willows along the banks too absorbing to leave much time for woods. It is a rich, leisurely meditation on man's view of man's condition and of its ultimate resolution, death, drawing on art, on literature, and, of course, on what we broadly call history.

"Death domesticated" was the first form of our more-or-less Christian, Western mortality, the dying tamed by the knowledge of its coming, by decent preparation for it, acceptance, and eternal rest. Soon this attitude was followed by a more self-conscious and individualistic death of self, the end not of one creature among many, going as all must go, but of me—myself. The new egocentrism went hand in hand with a new vision of judgment. The end evolved from a millenarian apocalypse, in

which the dead were resurrected to deliverance, into a last judgment which separated the just from the damned. After the twelfth century, judgment gradually gained ground over mere salvation, justice over charity; souls were weighed in the balance of Michael and awed by the flaming sword of Gabriel. Man found himself in a court of justice, mundane reality glorified beyond the tomb, which also emerged from the mass grave and charnel house. Soon the balance of heavenly justice would turn into an account book, a kind of individual *bulletin de travail* which souls could carry through the other world.

Individuality, and greater success in asserting it, led to a stronger sense of freedom, achievement, and success, and to greater frustration over the loss of their fruits. Death became deprivation. Life was fragile, the body corruptible; the decaying corpse provided a symbol of obsessive reality. This feverish reaction to a much more assertive, and now oppressive, mortality would, by the seventeenth century, turn to calmer grandeur, a baroque death re-integrated into an orderly, hierarchic society. Yet, through the promise of eternal life pierced the suspicion that nothing may lie beyond. Cultivated elites responded to this by turning toward nature, fearsome in its sepulchral vaults but also potentially reassuring; the urban masses, by neglecting their religious duties. On the eve of the French Revolution the greatest cemetery of Paris, that of the Innocents, was destroyed, the bones that rested in it were transferred to the city quarries henceforth turned into catacombs, and the spot was turned into a public square, all amidst popular indifference.

By that time a new attitude was taking over. In 1749 Thomas Gray's "Elegy in a Country Churchyard" marked the shift from fear and horror to serenity and sentiment. Mourning eliminated the mourner's passionate revolt, while the rites of death began to provide a screen between man and mortality. *Pace* Gray, it was not so much the churchyard as the cemetery that came to reign. Pious recollection of the dead, *one's* dead, would concentrate there. So would their graves, once scattered in churches, churchyards, or private holdings, their monuments and glorifying epitaphs (increasingly democratized) and their rights in real property reflected in the growing vogue of *concessions perpétuelles*. From indifference, the nineteenth century thus moved to a new cult of the dead (funerary monuments, mourning pictures) and of their tombs, which, from impersonal and transient resting places became personalized—ideally identified with one person or family only.

The dying still knew how to die, their death was still part of the life around them. As had ever been, death remained a social and, to some degree, a

public event. In the twentieth century this changed. Increasingly after 1914 society rejected death, denied it, sought to live, at least in towns, as if no one died any more. Dying became a grubby and embarrassing affair. Death had to be managed and cosmeticized. Man was dispossessed of his own death, deprived of the right to come to terms with it. The dignity of dying was abolished. Banished to a hospital bed, stuffed with drugs, bristling with tubes, the dying person no longer dominated his agony or end. And society ignored him.

This, *grosso modo*, is the tale that Ariès tells with an extraordinary abundance of images, quotations, comparisons—and much wisdom. One can take issue with all or some of the theoretical structure, or with the accommodation of theory and facts. But what would be the point? *Si monumentum requiris, circumspice*.

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DONALD F. LACH. *Asia in the Making of Europe*. Volume 2, *A Century of Wonder: Book 2, The Literary Arts*; Book 3, *The Scholarly Disciplines*. Chicago: University of Chicago Press. 1977. Pp. xxiii, 392; xii, 395–764. \$50.00 the set.

With “books” two and three completing its second volume, the *magnum opus* of Donald F. Lach and his collaborators takes leave of the sixteenth century. Actually issued in five separate volumes, with its goal two centuries ahead, the work has now reached nearly two thousand pages, illustrated with over three hundred plates. The method in books two and three of volume two differs somewhat from that in book one, *The Visual Arts* (reviewed by Bernard Smith, *AHR* 77 [1972]: 482–84). There, because of the nature of the subject, the 150 plates followed the text. Here, 104 equally fascinating plates are placed at intervals within the text to facilitate reference. Chiefly facsimiles of title pages, maps, and first European printings of Asian scripts, they—together with a bibliography of 165 pages—give a vivid impression of the wide scope of the work.

After introducing the Portuguese “heralds of empire,” especially Damião de Góis, Lach describes his own exhaustive examination of the holdings in sixteenth-century European libraries of works relating to Asia, emphasizing the importance of private collections such as that of Ferdinand Columbus. He then passes on to the overland transmission of Asian lore to Europe during the Middle Ages, emphasizing not only the Polos and other early travelers but also the Alexander ro-

mance and the transmission of elements of the Buddha story through the *jataka* tales and the legend of Barlaam and Jehosaphat.

The way is thus prepared for a thorough discussion of the role of Asia in Portuguese, Spanish, Italian, German, Dutch, and English sixteenth-century literature. Here, Lach not only deals with the important contemporary Latin works but shows how overseas expansion stimulated translations of these works, as well as translations from one vernacular to another and original works in the vernacular languages. He finds French thought particularly noteworthy. Not as involved in the discoveries as the Iberians and Italians, the French could view European expansion in Asia more objectively. Lach’s treatment of such major figures as Ariosto, Tasso, Rabelais, Montaigne, Marlowe, Shakespeare, and, above all, Camoëns, should be of great interest to teachers of comparative literature. Among the following chapters on the sciences and linguistics, historians will find of most value those on cartography and the transmission to Europe of knowledge of Asian languages.

Lach handles the vast mass of material, involving as it does estimates of the worth, interrelations, and influence of hundreds of books, with great skill. Readers are likely to consult Lach’s work on particular points; many will rely heavily on the few summary paragraphs that close each chapter. In future printings, these might well be set off more clearly from the preceding paragraphs, and could ultimately form the basis for an abridged edition of the whole work. More emphasis should be given to the vast difference between European empire in sixteenth-century Asia and in the Americas. Despite the contemporary rhetoric about conquest and rule, the Portuguese empire in Asia was primarily on the sea, a thalassocracy depending on a few fortresses and trading factories. In the sixteenth century, the parts of Asia that most influenced Europe were those least under European control.

Despite the references to the Levant, many readers, especially students, of these two “books” will be left with the impression that, once Vasco da Gama reached Calicut by sea in 1498, Asia almost ceased to take part in the “making of Europe” by the overland routes through the Middle East. Now that the work of the Danish scholar Niels Steensgaard has revived interest in the importance of the continuance of trade by land after the opening of the sea route, it would seem that the part played by the overland routes after 1498 in the transmission of ideas should receive more attention than it does here. As for minor matters, war galleys and oared frigates did not disappear from the Eastern seas with the advent of Albuquerque. The Portuguese were still using them in the seven-

teenth century. The controversies about Amerigo Vespucci's actual accomplishments deserved mention. References to Islam as a place (p. 410) and Venetian as a language (p. 22) may confuse young readers, and it is hardly remarkable that holdings in some sixteenth-century private libraries exceeded those of the library of Corpus Christi, one of the smallest Oxford colleges (p. 70). Misprints are extraordinarily few. The University of Chicago Press as well as the author are to be congratulated for one of the major scholarly achievements of the late twentieth century.

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W. WARREN WAGAR. *World Views: A Study in Comparative History*. Hinsdale, Ill.: Dryden Press. 1977. Pp. x, 223. \$9.95.

In postulating four world views that have dominated Western intellectual life since the seventeenth century, W. Warren Wagar is obviously viewing the world from a very high altitude, but such satellite pictures can be educationally useful as a first orientation. The last of his four divisions is the shakiest. "Rationalism," his term for the seventeenth and eighteenth centuries, may do, despite our qualms about lumping the Cartesian seventeenth with the more empirical eighteenth (in one sense the opposite of "rationalist"). Romanticism and then positivism certainly were potent structures of the earlier and later nineteenth century respectively, although the century is marked by a decreasing willingness to submit to such tags. But when everything since 1890 is swept into a basket labeled "neoromantic irrationalism," our skepticism about the value of such immense categories overflows. We can pick out Nietzsche, Bergson, expressionists, existentialists, etc., and perhaps agree to label them in this way; but what then are we to do about Valéry, Eliot, logical positivism, cubism, and structuralism? Wagar's model of four world-views, each neatly containing its era, stumbles over the fact that the world has been growing steadily more diverse and diffuse with each generation.

Of course, Wagar knows that his types are quite ideal and marks some of the exceptions. His sweep through the centuries of thought may seem cavalier but is often magisterial. It will both annoy and stimulate. He does not have time for the nuances. Unless qualified it is difficult to accept his dictum that the great British Victorians were complacently self-satisfied—one example of a judgment that will cause specialists to wince (p. 118). Nor, to take another case, will it do to lump Arnold Schönberg with Gustav Mahler as "irrationalist,"

(whatever that may mean in music). Like the Marxists' "bourgeois," Wagar's "irrational" obscures all distinctions, everything in the period being a priori an example of it—a night where all cats are black, as Hegel would have said. The reader is frustrated by a need to raise such objections, caused by excessive generalization, on almost every page.

But Wagar is not writing for specialists, and his crisply written, stimulating chapters should prove pedagogically successful in their very power to arouse debate. He has supplied a useful bibliography as well as an interesting preliminary discussion of just what "intellectual history" is. In conclusion, he offers a prognosis for the coming fifth world-view which is as provocative as the others, and I think the least convincing. Like other structures of intellectual authority, Marxism is in process of dissolution and has spread beyond narrow circles of political sectarianism only because of this demythologizing. It cannot contain all the complexities of a bewilderingly pluralistic intellectual society. The future is, of course, anybody's probably wrong guess and Wagar is entitled to his. But history may already have overtaken this latest fad which he wishes to make the orthodoxy of tomorrow. Of all guesses the best one seems to be that, so long as it lasts, Western thought will endure no single orthodoxy.

Intellectual historians will want to place this book on a small shelf reserved for attempts at sweeping synthesis, usually flawed (*quoque peccavi*), in that most exciting if elusive of subjects, the history of ideas. It will occupy a distinguished spot.

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W. B. GALLIE. *Philosophers of Peace and War: Kant, Clausewitz, Marx, Engels and Tolstoy*. (The Wiles Lectures.) New York: Cambridge University Press. 1978. Pp. x, 147. \$12.95.

MICHAEL HOWARD. *War and the Liberal Conscience*. (The George Macaulay Trevelyan Lectures in the University of Cambridge, 1977.) New Brunswick, N.J.: Rutgers University Press. 1978. Pp. 143. \$8.00.

As published lectures, both these books display the virtues of their genre: elegant and flowing style, clear organization, and balance between relevant detail and insightful analysis. The two essays are incidentally similar in that their authors are Oxbridge dons (Michael Howard at Oxford and W. B. Gallie at Cambridge), rough contempo-

raries, and delivered their lectures at British universities (Gallie at Queens University, Belfast, and Howard at Cambridge) during the same period (1976-77). More important, both treat their subject with critical yet sympathetic rigor and combine enlightened pessimism with cautious hopefulness; where they differ they are inclined to complement rather than contradict one another. Above all, they remind us that concern for war does not originate in our age and that much can be gleaned from the insights of previous epochs. These essays commend themselves not only to students of war or the specific thinkers analyzed but also to those concerned with the history of ideas.

Howard offers a useful starting point with his suggestion of a typology. The "liberal conscience" or liberals he defines as "all those thinkers who believe the world to be profoundly other than it should be, and who have faith in the power of human reason and human action . . . to change it." Conservatives he views as those "who accept the world as it unalterably is and adjust to it with more or less of a good grace." Determinists, including Marxists, are those "who see men as trapped in predicaments from which they can be rescued only by historical processes which they may understand but which they are powerless to control" (p. 11).

The conservative view prevailed until the eighteenth century and was espoused by Machiavelli, More, Grotius, Pufendorf, Vattel, Bacon, Locke, and Hobbes, who held that "for better or worse war was an institution which could not be eliminated from the international system. All that could be done about it was, so far as possible, to codify its rationale and to civilise its means" (p. 18). Erasmus was exceptional in his emotional denunciation of Mars as "the stupidest of all the poet's gods" (p. 14). During the eighteenth century the prevailing conservative interpretation was for the first time contested by thinkers such as Montesquieu, Rousseau, Quesnay, Turgot, and Adam Smith, who argued that war could and should be abolished by substituting the sane interests of reasonable men for the selfish ambitions of princes. As Gallie demonstrates, this argument was put most profoundly and compellingly by Kant, while Paine found a practical model in America, whose "system of peace" he contrasted favorably with the old European "system of war" (Howard, p. 29). The French Revolution sharpened and divided perceptions of war. For Paine it demonstrated that peace was ultimately a question of establishing democratic institutions. Yet the revolution produced both the first total war and its most perceptive analyst in Clausewitz; Gallie exposes Clausewitz's contradictions but demonstrates the utility of his basic notions of absolute (ideal total)

war and real (political or historical) war and his insight that war is a social rather than isolated phenomenon.

As liberalism succeeded during the nineteenth century, it split into opponents and proponents of war. The eighteenth-century faith that rational men could and ultimately would avoid wars was expounded by Bentham, Mill, Cobden, Bright, Say, and Comte and was popularized by peace societies; an essentially moral tack was meanwhile taken by the Quakers and Tolstoy, who, Gallie shows, attributed war in the first instance to evil governments but ultimately to original sin. Yet Paine's advocacy of just wars to achieve peace through democratic self-determination was extended by nationalists like Mazzini, List, and eventually Wilson to include national self-determination; war was also justified in Gladstone's eyes to defend "the public law of Europe." This contradiction within liberalism was most poignantly revealed during the 1930s when it sought at once to avoid war and resist fascism and during the 1960s when the just war stream ran into the sand of Vietnam. Marx and Engels cut this Gordian knot by conceiving war as its own necessary, inevitable, and final solution. Whether or not one accepts this view, Gallie argues persuasively that Marx and particularly Engels broadened our understanding of war by seeing it as a concomitant of the social structure, a feature of group rivalry, and a vehicle for historical change.

Howard and Gallie agree in concluding that none of their thinkers provides an unflawed theory of war and that all contradict themselves and one another. Yet Gallie contends that they also complement and correct each other, and Howard shows how ideas are adapted to changing circumstances. More important, they demonstrate the variety of precontemporary reflections on war whose greatest utility may be to stimulate contemporary debate. But debate is not solution, and neither author is optimistic about the prospects for eliminating war. They nonetheless subscribe to Kant's admonition that "it is our duty to act according to the idea of such an end [perpetual peace] even if there is not the least probability that it can be achieved" (Howard, p. 26). It is difficult not to respond.

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C. H. LEE. *The Quantitative Approach to Economic History*. New York: St. Martin's Press. 1977. Pp. vii, 117. \$10.95.

PATRICK O'BRIEN. *The New Economic History of the Railways*. New York: St. Martin's Press. 1977. Pp. 121. \$13.95.

It is now almost two decades since economists launched the "New Economic History," and it is by now clear that the quantitative methodology which has been dubbed "cliometrics" by its practitioners is here to stay. Historians must either direct their efforts away from the analysis of the "economic problem" in history, or they must confront the findings of the cliometricians head-on. These two little books are designed to aid those who choose the latter course.

Patrick O'Brien's *The New Economic History of the Railways* is an effective summary of the arguments about the impact of railroads on economic growth in industrial nations. The debate was touched off in 1964 by Robert Fogel's claim that the railroad was not "indispensable" to American growth. Fogel argued that the "loss"—he called it the "social savings"—if rail transportation were eliminated in 1890 would not have exceeded six percent of total product in 1890. His conclusion was not accepted without comment. Almost simultaneously, Albert Fishlow presented findings for 1859 which, while they reinforced Fogel's claim that the railroad was not indispensable, nevertheless suggested that the role of railroads in the economic development of the United States between 1860 and the end of the century was substantial.

Fogel and Fishlow were only the beginning. In addition to those who took issue with Fogel or Fishlow, studies appeared examining the impact of railways in England (Gary Hawke), Russia (J. Metzger), and Italy (Stefano Fenoaltea). O'Brien covers all this ground very deftly. Pushing aside to an appendix the debates over the empirical evidence, O'Brien examines what "social savings" means—in a static as well as a dynamic sense. His conclusion is hardly surprising: "As they stand, and on their own, estimates of social savings for England, America or Russia appear to convey only limited information about the importance of the railways compared to other innovations" (p. 96).

To his credit, O'Brien does not stop with this point; his journey through the maze of interpretations includes a clear, nontechnical discussion of the "linkages" through which the economic effect of rail investment and transport savings are transmitted to other economic sectors, as well as the tricky problem of how "consumers' surplus" affects the investment decisions on transportation.

Much of the success of O'Brien's text is that he focuses on a specific problem in examining the way in which the New Economic History analyzes problems. The inability to find a similar focus is a major shortcoming of C. H. Lee's *The Quantitative Approach to Economic History*. Though he does a nice job of outlining the usefulness of economic theory in his first chapter, Lee is much less successful in

demonstrating how quantitative techniques can help a neophyte cliometrician. His pedagogy seems to be based on "learning-by-doing." That is, Lee hopes the reader will capture the subtleties of econometric techniques by examining the results of a variety of studies. But neophytes will seldom see the subtleties when they have not yet grasped the basics. And, while his intentions are good, Lee provides little or no indication as to the theoretical postulates which underlie even the simplest of regression analyses. Lee does provide some nice summaries of methodological contributions from a wide range of studies, but he fails to develop a real background for applying abstract theoretical models—economic or econometric models—to historical problems. Those who are familiar with cliometrics will view the presentation as too simple; those who know little statistics will view it as too complex.

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MICHAEL WALZER. *Just and Unjust Wars: A Moral Argument with Historical Illustrations*. New York: Basic Books. 1977. Pp. xx, 361. \$15.00.

Michael Walzer's purpose is to "recapture the just war for political and moral theory" (p. xiv)—that is, he wants to increase the practical significance that the concepts of the just war and of humane behavior in war have for our times. His study is divided into five parts: "The Moral Reality of War," opposes the crude realism of "all's fair in war" with the affirmation that every experience in war has a moral dimension, whether openly acknowledged or not; "The Theory of Aggression" deals with the role of war in international relations; "The War Convention" outlines the formal and informal judgments by which governments, services, and societies determine what is permissible military conduct; "Dilemmas in War" discusses the tension between ethical behavior and winning; and the final section addresses "The Question of Responsibility" in policy, operations, and individual conduct.

It is not surprising that the book is less successful in developing a comprehensive modern theory of the just war than in analyzing specific issues and actions. The test of a general theory is how well it copes with difficult cases—not with obvious aggression or self-defense, but with those conflicts for which substantial justification may exist on both sides. Furthermore, if such a theory is to be influential, let alone truly effective, it must rest on an ethical consensus of considerable authority within each particular society, and probably also on val-

ues that are recognized throughout the world, or at least large parts of it.

Respect for individual life and belief in democracy are central to the author's arguments. But they are far from universal political values. Walzer even implies that democracy represents the greatest moral good: "the state (or government) established against the will of its own people, ruling violently, may well forfeit its right to defend itself even against foreign invasion" (p. 82). One problem with this proposition is that the facts it attempts to deal with may be elusive. For instance, would the USSR be a case in point? Is it even feasible to ask whether in 1917 the Communists gained power with or against the will of the Russian people, and make one's answer the basis of policy? The same kind of test is applied to some operational issues, for example, the author argues (p. 185) that guerrillas have different combatant rights according to whether or not the support they receive from "the people" is "voluntary." Again the practical difficulties of defining such a condition and establishing the truth, particularly during a conflict, are slighted. In a work concerned with humanizing actual behavior, rules that are intrinsically ineffective seem out of place.

The second half of the book is far more convincing. The chapters on terrorism and on reprisals, for instance, are excellent, as is the discussion of neutrality. The historical illustrations, mainly chosen from the last hundred years, are treated clearly and objectively, and the entire work—though in parts culturally parochial—commands respect for its attempt, by reflecting on the past, to find a more satisfactory and workable moral guide for the future.

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JOHN KEEGAN. *The Face of Battle*. New York: Vintage Books. 1977. Pp. 354. Paper \$2.95.

"Old, Unhappy, Far Off Things," the initial chapter of John Keegan's *The Face of Battle*, is "must" reading for anyone with even the remotest interest in military history. It should also be required reading for those of our colleagues who continue to tolerate us, with a smiling condescension, as "buffs" playing at historian—the substantial volume of scholarly military history published over the last three decades notwithstanding. As a thoughtful and unusually perceptive essay on the history of military historical writing, on its usefulness, and on the problems which confront the scholar venturing into this most difficult field, Keegan's book is not likely to be superseded for many years.

As for the battle studies themselves, whether by design or accident, the author has written what amounts to a symphony in prose. Beginning with the *forte* of Agincourt, he builds to a *fortissimo* at Waterloo, climaxes with the *sforzando* on the Somme, and concludes with the somber and almost certainly controversial *piano* of the final chapter, "The Future of Battle."

The account of Agincourt (October 25, 1415) lacks the conviction of the later studies, and the author has not fully used the available sources. But it is only fair to note that not many longbowmen, pikemen, or even men-at-arms are likely to have written to their families or friends after the battle; if any did so, their letters have not survived. The extant eyewitness accounts, such as the *Gesta Henrici Quinti*, are much too detached, and we shall never really know the personal feelings of the men who faced death on St. Crispin's Day, 1415.

By the early nineteenth century, not only were officers literate—to a degree—but there were "other ranks" who could write, and often write well. Thus when Keegan reaches the field of Waterloo (June 18, 1815), the narrative picks up tempo. We can almost see the English regiments, close-ranked in their squares, standing firm in the face of point-blank salvos from the French artillery; we can almost hear the musketry volleys, the cries and screams of wounded men and horses, and smell the acrid powder smoke. Perhaps the most significant impression that emerges is that of the "fog of war" that enveloped even ranking officers, who had but the vaguest notion of what was taking place elsewhere on the field.

This growing isolation of the individual on the battlefield is even more evident on the Somme (July 1–November 13, 1916). Keegan has limited his coverage largely to the first day of the offensive when "sixty thousand men paid forfeit for the plan that failed" (Basil Liddell Hart, *The War in Outline 1914–1918*, [1939], 135). But the build-up to July 1, 1916 is a masterpiece. Of particular intensity is the account of the recruitment of the "Kitchener," or "New Armies," and the potential for tragedy in countless villages and urban neighborhoods, when the "Pals" or "Chums" units climbed out of their trenches on that ghastly day. This is tragic drama as well as history.

The final chapter will certainly be challenged, and it may well be doubted that battle in the future will be so compartmentalized, even if it is fought out between major powers, as the author suggests. There are many trouble spots in the world where the logistical problems would be of such staggering magnitude as to preclude anything but the one-on-one type of warfare that is virtually as old as written history itself.

Keegan's study contains minor errors, to be

sure, but these cannot detract in any substantial way from his major conclusion that the study of battle "is not a study only for the sociologist or the psychologist, and indeed ought not perhaps be properly a study for either. For the human group in battle, and the quality and source of the stress it undergoes, are drained of life and meaning by the laboratory approach which social scientists practice. Battles belong to finite moments in history, to the societies which raise the armies which fight them, to the economies and technologies which these societies sustain. Battle is a historical subject, whose nature and trend of development can only be understood down a long historical perspective" (p. 298). It could not be put better than this.

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ISAIAH FRIEDMAN. *Germany, Turkey, and Zionism, 1897-1918*. New York: Oxford University Press. 1977. Pp. xiv, 461. \$29.50.

This book fills a lacuna in our knowledge of the history of the first twenty years of the Zionist effort. Although several fine studies have been published on the relationship of the young Zionist movement to Britain and on the role Britain played in espousing Zionist aspirations at that early stage, no corresponding study has existed about Zionism and Germany. Isaiah Friedman's thorough study ably fills this hiatus and shows for the first time the considerable extent of support the Zionist movement received from the German government. This support was especially important since Germany was closely associated with, and in World War I became an ally of, Turkey, the suzerain power in Palestine until the end of the war. Thus, Germany was in a position to intervene with Turkey in favor of the Jews who lived in Palestine at the time, and Germany did, in fact, use its influence for their benefit. As the author shows, had the Germans not persistently intervened with the Turkish government, the Jewish community of Palestine would not have survived.

It is interesting to note that on November 30, 1917, a draft declaration closely paralleling the famous British Balfour Declaration issued exactly four weeks earlier, was submitted by Matthias Erzberger, prominent leader of the German Center Party and director of the Intelligence and Propaganda Bureau, to Richard von Kühlmann, German ambassador to the Sublime Porte. It begins, "The Imperial Government is favourably disposed to the aspirations of the Zionist Organization to create a Jewish National Home for the Jewish

people in Palestine and is willing to promote this aim energetically" (p. 345).

The difference, of course, was that while the Balfour Declaration was actually issued in the name of His Majesty's Government, the German declaration never went beyond the desk of the ambassador. Friedman's book is full of such hitherto unknown details, and it conclusively shows that the German Zionists were correct in their policy of maintaining as close a contact with the German government as their British colleagues did with that of Britain.

It is regrettable that such an important study should suffer from sloppy proofreading. In the English translation of the Erzberger draft, as a comparison with the German original printed in an appendix shows, an entire phrase has been omitted. The name Fuad Bey is consistently misspelled as "Faud." In the bibliography, especially in the listings of Hebrew titles, misspellings are numerous.

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ROBERT SKIDELSKY, editor. *The End of the Keynesian Era: Essays on the Disintegration of the Keynesian Political Economy*. New York: Holmes and Meier. 1977. Pp. xiv, 114. Cloth \$17.50, paper \$7.50.

Economists appear to agree that their discipline is in full crisis. Superficially this presents itself as their seeming inability to solve the problem of "stagflation." The problem, however, is much more fundamental. Keynesians, monetarists, and institutionalists, to name some, have of late been unable to find much common ground. This confusion appears all the more remarkable in light of the great technical progress made within the discipline during the post-1945 period, chiefly the use of sophisticated mathematical models and the harnessing of computer technology. This has produced an increasing division of labor which has further reduced the area of common ground. The disintegration of Keynesian economics is one aspect of this larger problem.

By bringing together thirteen brief essays, which first appeared in the *Spectator*, Robert Skidelsky hopes to demonstrate that the crisis of Keynesian economics can only be understood if viewed in a wide historical perspective. Skidelsky begins by rooting Keynes's "anti-Puritan" economics in the esthetic tradition of Bloomsbury. In a second essay he reminds us that Keynes's central purpose in the 1930s was to provide a capitalist alternative to the then encroaching fascist and communist models. John Vaizey places Keynes firmly within a Cambridge tradition of economics stretching from Alfred Marshall to Piero Straffa. David Calleo and

Marcello de Cecco emphasize that Keynes had dealt with the peculiar problem of a nation burdened with excess saving and an overdeveloped financial structure—a situation that was radically altered by American power and wealth after 1945. Peter Lilley puts forth the claims of Friedman and Hayek as successors to Keynes. Samuel Brittan notes that Keynesian management of the economy might have been possible in a paternalistic society run by experts, such as Keynes, but not in the more democratic postwar period. This theme is taken up by Aubrey Jones with his argument that special interest groups have institutionalized inflation. Stuart Holland claims that Keynes became the godfather of a generation of postwar labor politicians but that more recently they have turned to the younger Marx. Robert Lekachman, however, still sees a Keynesian salvation if only we follow the Keynes of the “socialization of investment” and the redistribution of income. J. T. Winkler attempts to persuade us that the future will be neither capitalist nor socialist, but corporatist. Harry Johnson reminds us that Keynes’s legacy has been problematical for the developing world. Finally, Geoffrey Barraclough faults the postwar generation for promoting an anti-Puritan ethic of consumption which has needlessly squandered finite resources. He concludes that the “Keynesian generation . . . should put its head in the gas oven, while there is still gas to go round” (p. 111).

These essays do not pretend to offer a new synthesis on Keynes and the end of the Keynesian era, nor do they embody detailed research, but they do suggest the scope of the problem that a much needed study of the end of the Keynesian era ought to treat. Unfortunately, no critical bibliography has been included. The major flaw, I believe, is the volume’s failure to point out that it is not only Keynesian economics that is disintegrating. For a statement of the larger problem the reader might turn to Walter W. Heller’s 1975 address to the American Economic Association.

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THOMAS S. KUHN. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press. 1977. Pp. xxiii, 366. \$18.50.

The name of Thomas S. Kuhn is appropriately associated with the belief that essential to the progress of science is a tension between conservative and innovative drives. This belief was among the theses defended in Kuhn’s famous book of 1962, *The Structure of Scientific Revolutions*, and had been outlined three years earlier in an article that

is now reprinted as the title essay in *The Essential Tension*. This collection of fourteen of his contributions to the theory and historiography of science is well named: tensions of one kind or another define much of the book. Whether he is discussing the relation of internal to external history, of logic to psychology, of mathematical to experimental science, of justification to discovery, of philosophy to history, of art to science, or of convergent to divergent thinking—to name a few of the binary relationships prominent in Kuhn’s pages—Kuhn seeks always to specify the levels at which parties to a relationship are relatively autonomous and at which they are interdependent. In a previously unpublished paper on history of science and philosophy of science, for example, Kuhn adamantly opposes the amalgamation of these two enterprises, while he is equally firm in demanding sustained interaction; each discipline would be weakened by too full a relaxation of the tension between them.

“Objectivity” and “subjectivity” are featured in a second new paper, “Objectivity, Value Judgment, and Theory Choice.” In answer to complaints that his account of scientific change renders too subjective the choices scientists make between competing theories, Kuhn reminds his critics that the choices he attributes to scientific communities are based on discussion within those communities (even when participants understand each other only in part) and not on the undiscussable, more properly subjective tastes of individual minds. The standards that inform these discussions (accuracy, simplicity, fruitfulness, etc.), Kuhn argues, are best seen as values to which the community assigns shifting weights, rather than as invariant rules in the following of which objectivity supposedly consists. Neither the incompleteness of communication, the depiction of standards as values, nor the variability of these values mark the subjective limits of objectivity, Kuhn adds; they instead partake of the reality that the term objectivity denotes. This important essay is not without elusive aspects, but it considerably clarifies Kuhn’s position and will amply instruct anyone interested in the philosophical implications of *The Structure of Scientific Revolutions*.

What Kuhn has to say about “Mathematical Versus Experimental Traditions in the Development of Physical Science” will probably attract more attention from historians than will any other piece in *The Essential Tension*. As the most recent (1975 in French, 1976 in English) of the twelve reprinted essays, it will be new to many readers who have already assimilated such other of Kuhn’s theoretical papers as “The Function of Measurement in Modern Physical Science” (1961) and “The Relations Between History and the History

of Science" (1971). This article speaks to our sense of what "science" has been as a historical presence in the West and thus promises to influence teaching and scholarship at points of basic conceptualization. Kuhn distinguishes sharply between a group of specialties (astronomy, harmonics, mathematics, optics, and statics) that shared, since antiquity, a single mathematical tradition, and a second group of more experimental specialties (beginning with magnetism and electricity) that have established themselves since the early seventeenth century. While conceding that the second group made rudimentary calculations and that the first gathered easily available data, Kuhn argues that these two traditions were so distinctive through the end of the eighteenth century that Newton, virtually alone, can be said to have participated unambiguously in both. In this perspective, the problem of the origins of the Scientific Revolution needs to be reformulated; the transformation of the classical, mathematical sciences and the emergence of the experimental, "Baconian" sciences are not to be confused, however related both developments undoubtedly were to common conditions in the culture and society of seventeenth-century Europe. Kuhn neither defends nor offers a comprehensive explanation for the Scientific Revolution, but, by providing vital specificity for our definition of that revolution, he tells us more about it than have many of those who have sought to explain it.

That the sciences are both "one" and "many" may be a truism, but Kuhn's determined, often brooding pursuit of this insight lies behind many of his most distinctive and controversial contributions. Central to his achievement in *The Structure of Scientific Revolutions*, for example, was the breaking down of the monolithic concept of objectivity, but no less central to that achievement was the attribution of a single structure to the multiplicity of changes that mark the growth of scientific knowledge. This arresting combination of claims is emblematic of the tension between particularity and universality that sustains this anthology of 1977 and that is essential to Kuhn's *oeuvre*.

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ALFRED D. LOW. *The Sino-Soviet Dispute: An Analysis of the Polemics*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1976. Pp. 364. \$18.50.

There is much to be said in favor of this book. It deals with a subject of obvious importance, inasmuch as a war between the two great Communist adversaries has been a realistic possibility since 1969. It is remarkably comprehensive; the author omits few significant aspects of the Sino-

Soviet dispute, although his emphasis falls heavily on the public polemic over ideological and political issues. Alfred D. Low has clearly done a great deal of work in original sources—meaning those versions of the Soviet and Chinese press and radio that are available in American academic libraries, and these are adequate for most purposes. He has made a valiant and generally successful effort to understand and convey his topic, which is noted for its obscurity and complexity as well as its importance.

The study, however, has a pervasive shortcoming, a degree of superficiality. Low summarizes the propaganda of each side without much of an effort at content analysis in depth. Inasmuch as the emotions of both sides are fully engaged in their mutual dispute and their elites are among the most enthusiastic liars on earth, their propaganda is bound to fall considerably short of conveying a true picture of attitudes and policies, to say nothing of facts. The polemic has been analyzed by others besides Low, sometimes with considerable sophistication, but he appears to have made little use of their labors even though they are cited in his bibliography, which was evidently prepared by his research assistant.

The superficiality is greatest when the analysis deals with state issues, such as the menacing border dispute, rather than with ideological and political polemics. An especially strange lapse appears on page 227, where the author cites *Peking Review* for March 7, 1969 as containing a document dated March 13 referring to an incident of March 15; this is indeed history in reverse. The analysis (pp. 280–81) of Lin Piao's report of April 1, 1969 to the Chinese Ninth Party Congress, insofar as it dealt with the border dispute, misses the main point. That point is that in the course of his blustering, Lin conceded that Peking would take the old "unequal" boundary treaties "as the basis" of a new one and therefore was not actually claiming any great amount of territory currently held by the Soviet Union.

Although the book was published in 1976, Low's coverage, which is chronological, ends about 1973. A number of interesting recent developments, such as the impact of Mao Tse-tung's death on Sino-Soviet relations and the growing importance that military factors in the dispute seem to be assuming, are therefore not treated.

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ELIHU KATZ *et al.* *Broadcasting in the Third World: Promise and Performance*. Cambridge, Mass.: Harvard University Press. 1978. Pp. xvi, 305. \$15.00.

This is a convenient summary of broadcasting in some developing countries. It provides a starting

place for those interested in broadcasting in other parts of the world but who lack background in such studies.

From ninety-one countries scaled from the most to least developed, the authors chose eleven for detailed study. They gathered other questionnaire information from ministries in twenty-five countries and broadcasting systems in twenty-eight countries. But they assert that they were able to find "good primary source material on 59 countries" (p. 283).

The major sections of the book review the broadcasting systems, their programming, and their interaction with their governments. Yet there is too much unsaid, and confusing points leave this a flawed volume. For example, the various references to Vietnam and South Vietnam should have been straightened out, since this book was published in 1978. A reference to U.S. programming claims that special programs have disappeared, giving way to series (p. 164); in fact, there are more than 300 prime-time entertainment specials a year, or about one a night. The authors argue that a response rate of about one in four for the questionnaires noted above is "usual." That is not true even for surveys of the general public. On two occasions I have gotten much higher responses by writing to broadcasting agencies and ministries with follow-up letters. Some items cry out for explanation: a description of programming in Peru states that "the news, too, featured baroque showmanship" (p. 73). Examples would help.

As part of the work, a 1,500 item bibliography was compiled but does not appear in the book. This lack seems to underlie much of the study. While this is a helpful and generally up-to-date summary, those who really want detailed information on broadcasting in even the case study countries should also look to volumes by Walter Emery, Sydney Head, John Lent, and regular summaries by UNESCO.

A more serious lack is that the book does not present any overall theory or model to help us understand the various types of broadcasting organizations, the relation of these to types of government and societies, and their effects on development, programming, and mass-communications audiences. Very useful are ten tables that give information on all ninety-one countries.

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ANCIENT

WILLIAM Y. ADAMS. *Nubia: Corridor to Africa*. Princeton: Princeton University Press. 1977. Pp. xxiv, 797. \$35.00.

Nubia, William Y. Adams points out, has generally been regarded as a peripheral aspect of Egyptian history. To remedy this, he has coupled his considerable first-hand experience as an archeologist in the region with broad reading in the scholarly literature to develop a "Nubiocentric" (alas) portrait of its history from prehistoric times to the present. Moreover, it is a point of view with distinct theoretical underpinnings. While the results are flawed in several respects, Adams has produced a book that is often informative, stimulating, and, despite its excessive length and often turgid style, interesting; he certainly merits praise for undertaking some needed reconsiderations of long-held views and predilections.

Adams, an anthropologist and archeologist, is, quite understandably, at his best in his examination of the archeological record, especially when he rethinks the earlier work in Nubia on the basis of the results of the extensive excavation carried out in conjunction with the construction of the Aswan High Dam. While some archeologists will no doubt find sufficient grounds for disagreement, Adams knows the archeological materials and interprets them with a large measure of good sense, restraint, and open-mindedness. While I am not persuaded by his idea that the Nubians reverted to a pastoral way of life in the third millennium. I think it an interesting approach to the difficulties of the evidence.

The book is a good place to go to find out what we know of the material remains of Nubian culture. In addition, Adams uses the evidence effectively to counter some of the more light-headed notions of earlier generations. For example, he has made a strong case for cultural evolution and continuity in Nubia against those who have usually supposed that major cultural changes reflect the appearance of new peoples in the area. Adams is opposed to the sort of determinism that is contemptuous of the forces for change operating within a culture, the kind of mind that has to postulate a "dynastic race" for Egypt's early rapid cultural growth or ancient astronauts to teach the Egyptians how to build pyramids. Adams is also leery of glib generalizations about race, especially in early Nubian history, and, quite properly, prefers to focus on culture. In his view the Nubians were a distinct cultural entity that found it difficult—often impossible—to sustain and develop its own cultural configuration in the presence of the Egyptian colossus to the north.

Where does the book fail to satisfy? Not surprisingly, the problems arise when Adams adopts the role of historian. In all fairness to him, he is aware of the problem; indeed, Adams is quite clear that anthropologists are generally deficient in this role. In an engagingly self-conscious epilogue, he writes that anthropologists "must give serious attention

to such matters as market economies, stratified societies, bureaucratic and feudal politics, military establishments, state ideologies, and all other diversified aspects of civilization. . . . We must add history to prehistory . . ." (p. 680). To which I hear a chorus of historians saying "Amen, Brother Adams, Amen!" Historians have their own vulnerabilities.

But Adams's practice of the craft is another matter. For one thing, he is unduly suspicious of the early textual evidence, because it is Egyptian—the Nubians themselves left no written records before the eighth century B.C.—and the Egyptians, in addition to being ignorant and ethnocentric, were also given to hyperbole and embellishment (p. 140). Practicing historians have long since become inured to the fact that some sources lie or exaggerate and others are ignorant, but they have fortunately resisted throwing out the grain along with the chaff. We would be in a sorry state were we to rule out the Greek sources for Roman history on the grounds that they were prejudiced, inaccurate, or embellished. To make matters worse, Adams must rely on translations; he sometimes chooses the oldest, or the worst, or the most misleading, or, worse yet, paraphrases. Thus his desire to be cautious about racial designations is somewhat undermined by his use of a translation of the Sen-usert III Semneh stela which quite gratuitously translates the Egyptian word for Nubians with "Negroes" (p. 185). Likewise, he refers to some Nubian leaders contemporaneous with the Egyptian Sixth Dynasty as "kings" when in fact the text says nothing of the sort (p. 158). Nor does he read the translated texts very carefully. He asserts that the Egyptians made no effort to patrol the desert flanks of their Nubian fortresses; the evidence of the Semneh Despatches is that they sent out patrols along the desert edges and sought intelligence about what was going on in the deserts. In sum, Adams has not been able or willing to exploit the Egyptian texts fully.

A second problem, one not uncommon among archeologists, Egyptologists, and some historians, is the matter of historical interpretation. The boundary between description and explanation is not always kept in mind. The models for interpretation are sometimes ill chosen; for example, his enthusiasm for Toynbee and Sirokin seems to get in the way rather than aid Adams in his rethinking of the issues. His concept of ancient Egyptian imperialism is simplistic. In the epilogue, he has a discussion of what he perceives to be cycles in Nubian history that is easily as naive as Adams suspects; Thucydides would blush for him. I have the sense that Adams has the beginnings of the idea of historical explanation, but needs to think that through as well.

Lastly, there is the problem of his dependency on scholars in the various periods or fields in which Adams is not expert. That can be a tangled thicket for anyone, and, as one might expect, he sometimes comes out badly scratched.

For all the above criticism, I applaud Adams's attempt. He has performed a service by opening up an interpretative debate. Although the book may be heavy going for some, educated lay readers will find it valuable; scholars, if they are open to it, will find it stimulating. Best of all, Adams has established the individuality of the Nubian experience.

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R. E. WYCHERLEY. *The Stones of Athens*. Princeton: Princeton University Press. 1978. Pp. xviii, 293. Cloth \$25.00, paper \$12.50.

Although the topography and monuments of ancient Athens have been studied by armies of scholars, the standard study of the city has been Judeich's *Topographie von Athen* published almost fifty years ago. The only survey in English was I. T. Hill's *The Ancient City of Athens*, published in 1951. R. E. Wycherley's *The Stones of Athens* supersedes the later volume and will become the basic survey, although it is not strictly a survey, being more a collection of essays and omitting or briefly mentioning a number of sites. Judeich's work remains indispensable for the specialist.

The author discusses ancient Athens in twelve lively, well-illustrated chapters. A study such as this can be undertaken from a historical point of view, but Wycherley follows a more or less topographical scheme. It is a comprehensive design, dealing both with specific portions of the ancient city as well as particular subjects appropriate to a study of any city. Parts of ancient Athens have already been exhaustively studied and relatively widely discussed in print, such as the Acropolis and the Agora, and these of course find their places in the text. However, many lesser-known areas and buildings are also discussed, including such general topics as the walls, houses, streets, water supply, shrines, and theaters. The author stays within the confines of the ancient city but with some digressions to famous gymnasia, philosophical schools, and recently discovered country houses. A valuable chapter summarizing what is known of the Peiraeus is also included. This adjunct to Athens, apparently laid out by the famous Hippodamos, is not well known to the English reader, and the brief account given here is valuable. A postscript, "The Stones," is a discussion of

the different types of stone used for building in Athens and is a particularly useful attempt to bring some order into the complicated field of terminology. Unfortunately, the title of the book itself can mislead a reader into thinking that it only concerns the subject of this one chapter.

The author, Emeritus Professor of Greek at the University of Wales, has been associated with the Agora excavations and has published a collection of literary testimonies to the area. His approach in this work is also from the literary point of view, which is entirely appropriate for this study designed as an overview with the general reader kept firmly in mind. It is a welcome change from a more technical, archeological treatment. The peculiarities of ancient Athens, the startling mixture of old and new, sacred and profane, are rightly emphasized by the author who is able to convey brilliantly the character of the city in the fifth and fourth centuries B.C. The emphasis on "Classical Athens," no doubt occasioned by the literary approach, is a weak point, for this reviewer at least. Roman Athens receives only passing notice, which is a shame, and the later history of the city is hardly mentioned, although the essential continuity of ancient and modern Athens is properly stressed.

This is an excellent book, long overdue, and one which will rapidly become a basic text in college courses dealing with urbanism in general and the topography and monuments of Athens in particular. The nondogmatic approach to complicated problems together with new evidence not previously easily available makes *The Stones of Athens* a joy to read.

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CORNELIUS C. VERMEULE III. *Greek Sculpture and Roman Taste: The Purpose and Setting of Graeco-Roman Art in Italy and the Greek Imperial East*. (Jerome Lectures, Twelfth Series.) Ann Arbor: University of Michigan Press. 1977. Pp. xii, 137. \$18.50.

Although there are no official statistics, it is probably true that the majority of the classical sculptures preserved today are copies or adaptations of original Greek statues executed by copyists during the Roman period. Throughout most of the past century of archeological research in the classical field, scholars have studied these copies primarily for the purpose of identifying and reconstructing lost Greek originals. Only recently have studies begun to appear which are concerned with the

significance and use of the copies in the time when they were made (for example, Paul Zanker, *Klassizistische Statuen* [1974]). Cornelius C. Vermeule III's book, originally given as the Jerome lectures in Rome and Ann Arbor, continues this new trend.

Vermeule's concern is not so much with the significance of individual copies as with how groups of copies were used to decorate temples, villas, baths, fountain houses, theaters, and so on during the late Republic and the Empire. The first two chapters discuss the nature of the copying industry and the principles on which ancient sculptural groups were designed; the third chapter surveys the evidence for how groups of copies and adaptations were displayed in Italy and the Latin West; the fourth chapter summarizes similar evidence for Greece, Asia Minor, and the East; and an epilogue analyzes the depiction of sculptural groups on Roman coins.

What Vermeule's study suggests is that one of the chief appeals which copies of Greek works had for the Romans was their decorative effect in architecture. Arrayed along porticoes and arcades or in the niches of a multi-tiered facade, such statuary helped to articulate and enliven buildings. Their function was in many ways similar to that of columns and pilasters.

Since symmetry was all important in Roman architectural decoration, the copying industry put emphasis on works that could counterbalance one another. The surprising frequency with which copyists created "mirror images" as well as direct copies of Greek works and then used the resulting pair of statues to frame sculptural groups is one of the revelations of Vermeule's study. Another is the frequency with which duplicate copies were exhibited in the same architectural setting. Apparently such duplicates not only helped to balance a design in Roman eyes but also challenged the Roman connoisseur to make subtle critical comparisons.

The book is a product of thorough scholarship, is very well documented, and its text is concise and easy to read. Although the sculptures with which Vermeule deals may not be among the most attractive or significant from the ancient world, his study is nevertheless interesting and valuable for two reasons. First, it helps one to appreciate the effect of a large corpus of Graeco-Roman sculpture in its original context. Second, it makes clear that the groupings into which this type of sculpture was arranged have had an influence on architectural design (as Vermeule's glances at Lansdowne House and the Boston Public Library demonstrate) up to very recent times.

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ROBIN BIRLEY. *On Hadrian's Wall. Vindolanda: Roman fort and settlement.* (New Aspects of Antiquity.) New York: Thames and Hudson. 1977. Pp. 184. \$22.50.

Robin Birley, Director of Excavations of the Vindolanda Trust, has published a thorough, well-illustrated, and readable account of the site, first systematically excavated by his father, Eric Birley, beginning in 1929. Vindolanda lies a mile south of Hadrian's Wall in the Tyne Valley of Northumberland, west of Corbridge and Hexham on the Stanegate, and two miles southwest of the great Roman fort, Housesteads, on the Wall. Vindolanda consists of the remains of forts and of an extensive civilian settlement (*vicus*) adjacent to them, all carefully excavated and studied by the Vindolanda Trust in an exemplary archeological enterprise. Apart from the merit of his report, of interest to the professional and lay public alike, Birley has presented a model account of the conservation of an ancient site achieved through effective cooperation among amateurs, student volunteers, and a resolute archeological staff, devoted solely and without limit of time to the complete and systematic recovery of Vindolanda.

Birley traced five periods of occupation at the site from shortly after Agricola's military operations in northern Britain and Scotland, ca. A.D. 80 to ca. 400 when the Romans disappeared. Excavations revealed that a fort was constructed at Vindolanda in the early 80s; it was subsequently strengthened ca. 95 and occupied by the First Cohort of Tungrians, serving on the Stanegate which marked the Roman frontier before the Wall was built. Vindolanda was abandoned ca. 125-60 when the garrison moved, probably to Housesteads, but it was reconstituted after 163 when a new stone fort was erected holding a cohort garrison, probably the Third Gallic Cohort; a second stone fort replaced the first ca. 270 and was, in turn, extensively reconstructed after 367.

Civilian settlements were attached to these forts containing soldiers' families, merchants, and artisans in an area several times larger than the forts themselves where strip and corridor houses, an inn or *mansio*, and a bath house have been found. Finds from the third century *vicus* indicate that the inhabitants enjoyed an abundant food supply with a large amount of meat in their diet, possessed substantial personal and household articles, and worshiped the northern British variants of Celtic deities.

Although the Vindolanda forts and their attendant *vici* are important because of the evidence they provide about the development of communities around the northern strongholds in the second and third centuries, the most spectacular

results come from the excavation of the pre-Hadrianic wooden forts. These difficult excavations reveal the earliest stages of Roman occupation and have produced extraordinary finds: varieties of leather goods—especially shoes—have survived, largely made of cowhide, and with them tools for tanning and working leather; textiles have also turned up, made of wool in all the standard Roman weaves, including one sample with the broad purple stripe, the *clavus*, the distinctive badge of the Roman aristocracy; and most spectacular of all, more than two hundred writing tablets, dating between 90 and 125, have been found. Some are stylus tablets, common in the Roman world, but the majority are made of thin strips of lime wood with carbon ink writing in cursive; these were preserved beneath the water table but require desperate efforts of conservation once exposed to air.

With the aid of infrared photography, A. K. Bowman and J. D. Thomas have deciphered many of the wooden tablets and divided them into two categories, private correspondence and accounts or inventories, related to the first garrison of Tungrians at Vindolanda. Not only is the content of these tablets of prime historical importance, but the writing itself has great value because it is in cursive, written by more than forty hands, and in phonetic spelling which gives evidence of Vulgar Latin in the late first and second century. Such tablets are almost unknown outside Egypt so that the discoveries at Vindolanda not only inform us about Latin popular speech on the frontier but also illuminate the dark history of Roman cursive before its appearance on late antique manuscripts.

By all accounts the Vindolanda Trust has achieved much, and it promises much more.

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T. J. LUCE. *Livy: The Composition of His History.* Princeton: Princeton University Press. 1977. Pp. xxvii, 322. \$18.50.

The structure of Livy's *History* has frequently been studied. T. J. Luce takes up this question in his first chapter; the next three chapters examine books 31-45, to demonstrate that they are structured in pentads; there follows a chapter on Livy's use of sources which profitably attacks the assumptions of much Livian source-criticism; chapter six examines Livy's working methods, using earlier results to argue that Livy prepared his material in blocks which, when written up, would fill units of fifteen books; a final chapter discusses Livy's concept of Roman national character to show that he had "a developmental concept of history" (p. 294).

In the first chapter Luce usefully summarizes the work of his predecessors. He notes the absence of consensus, but feels that "on balance . . . the scales are weighted in favor of a pentad-decade pattern" (p. 17). The fact that, as far as we know, the *History* consisted of 142 books is awkward for this conclusion, and logically enough at one point (p. 24n) he thinks it plausible that "Livy intended to end with the death of Augustus in a projected Book 150." This intention could be formulated with confidence only after Augustus's death (A.D. 14), and Jerome's date for Livy's death is A.D. 17. It is therefore supposed that Livy was writing the last books at the time of Augustus's death. Luce does not consider the possibility that Jerome's date is five years too late, though he does accept this possibility for the date of Livy's birth. In any case, he confesses that he "can find no pattern in Books 121-142" (p. 24n). Any investigation of the *History*'s structure is hampered by the fact that we possess only inadequate summaries (*Periochae*) of most books, and the *Periochae* of the concluding books are particularly sketchy. It is perhaps not surprising, therefore, that Luce's arguments for a systematic structuring of the entire work are seldom more persuasive than those of his predecessors.

The nub of the work is the analysis of books 31-45, in which Luce advances a persuasive case for the structuring of this part of the *History* in pentads, and for the planned composition of individual books. He shows how Livy abridged or expanded his material to suit his plan, and how in adapting his sources he was sometimes led into muddle and error, especially over chronology. These chapters and the one on his working methods further our understanding of Livy. It is worth asking, however, whether Livy can have planned his work in quite the same way in those later books where he could no longer rely on Polybius. The breakdown of the pentadic structure after book 45, often posited, may well be due to the lack of a reliable major source.

Detailed discussion of individual points does not always make for easy reading, and there is some repetition. I also miss consideration of how the narrative of particular incidents was composed. Nonetheless, the work should be welcomed for its substantial contribution to Livian studies.

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MEDIEVAL

ROBERT SOMERVILLE. *Pope Alexander III and the Council of Tours (1163): A Study of Ecclesiastical Politics and Institutions in the Twelfth Century*. (Center for Medieval and Renaissance Studies, UCLA, number 12.)

Berkeley and Los Angeles: University of California Press. 1977. Pp. xi, 110. \$8.50.

Pope Alexander III spent three years (1162-65) in France as a refugee from Frederick Barbarossa, who was supporting a rival pope, Victor IV. In September 1162, the Victorine party celebrated a council in Burgundy. Within a month, Alexander had met with the kings of France and England and had laid plans for a counter council that would demonstrate the strength and unity of his supporters.

This council of the Alexandrine obedience was duly celebrated in May 1163 at Tours, where some 650 prelates—including 17 cardinals, about 120 bishops, and 414 abbots—were mustered from the British Isles, France, Spain, and Lombardy. Latin Christendom outside the Empire manifestly stood with Alexander, and by avoiding controversy at the council, he showed that they were solidly united as well. Although the chief Victorines were excommunicated at Tours, as a conciliatory gesture the emperor was not. The assembled fathers indeed seem to have done very little; the fact that they were united under Alexander was the main point of the meeting. To be sure, the pope took the occasion to publish at least eight canons that became a permanent part of canon law, but he might just as well have issued them as decretal letters. Nonetheless, Tours was one of the greatest papal councils of the twelfth century, and as such it deserves the attention lavished on it in this monograph.

Robert Somerville's aim is to collect all the sources and to evaluate them in the context of modern scholarship. He edits no old text and only one that is new—a list of the participants. Instead, in successive chapters he systematically discusses each of the council's salient features: its occasion; the sermons preached; disputes over York's claims to primacy over Canterbury and Scotland; the twenty-two canons attributed to the council; the traces of other conciliar business; and, finally, the council's place in the history of the schism.

In this small book, Somerville traverses a wide range of learning with competence and shows himself to be a master synodographer. Though his style is undistinguished and at times even obscure, his concision is admirable. The author certainly attains his limited goal: all the facts about the Council of Tours are now available and up to date. One can only hope that eventually all the medieval councils will receive comparable treatment.

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DONALD E. QUELLER. *The Fourth Crusade: The Conquest of Constantinople, 1201-1204*. (The Middle

Ages.) Philadelphia: University of Pennsylvania Press. 1977. Pp. xi, 248. \$17.00.

Historians have found the fourth crusade (1202–04) more perplexing to reconstruct and to understand than almost any other crusading expedition. The problems do not arise from a scarcity of sources, for this expedition was better documented than most others. We have narrative accounts of the fourth crusade written both by a rank-and-file member of the army (Robert de Clari) and by an eminent leader of the army (Geoffroi de Villehardouin). In addition there is a sober, well-informed account of the whole extraordinary business written by a contemporary observer and enemy of the crusade (Nicetas). Beyond this, the fourth crusade's activities are the subject of numerous letters and other nonnarrative documents. As medievalists look at things, the documentation of the fourth crusade is rich—not embarrassingly rich, perhaps, but more complete than for many other crucial episodes of the Middle Ages.

The historian's perplexities about the fourth crusade arise, instead, from the fact that this expedition abandoned its intended target in the Holy Land to attack the Adriatic city of Zara, which belonged to the Christian king of Hungary, and then Constantinople, the capital of the Christian Byzantine Empire. The perennial puzzle that historians of the fourth crusade must solve, then, is how to account for the crusade's deflection from its course. Donald E. Queller proposes a fresh answer to the old puzzle; and in this well-written, abundantly documented book he sets forth his theory and the evidence upon which he bases it.

Queller divides previous accounts of the fourth crusade into two groups: the treason-theory histories and the theory-of-accident histories. Treason theorists view the deviation of the crusade as a result of the machinations of the Venetians, who used the naive northern European crusaders to secure control of Zara and Constantinople, and thus gained economic and commercial advantages for Venice. Theory-of-accident accounts describe the deviation of the crusade as a result of a series of fortuitous circumstances, especially the arrival on the scene at a critical moment of the son of the deposed Byzantine emperor, Isaac Angelus. The opportunity thus presented, according to accident theorists, lured both the Venetian leaders and the leaders of the crusade to turn the expedition aside from its original goal in order to take advantage of the unexpected chance to gain control of Constantinople.

Queller's theory is a variant of the modified theory of accidents. Carl Norden and others have proposed earlier modifications of the accident theory, but Queller's account differs in its emphasis

upon the vital importance of the crusading leaders' gross exaggeration of the numbers of participants in the crusade. Their over-estimate, as Queller sees it, led the chiefs of the expedition to agree to a transportation contract with Venice that committed both the Venetians and the crusaders to far greater financial outlays than could possibly be recovered from the resources of the expedition. The resulting financial pressure upon both the Venetians and the crusade's leaders led them to embrace whatever opportunities they could find to salvage their financial situation. Queller also plausibly accounts for the rifts within the expedition between the leadership and the rank-and-file crusaders. The leaders had to face the practical financial problems and were committed to keeping the expedition intact. The rank-and-file members of the expedition were less immediately affected by the financial problems and viewed reaching the Holy Land as a more important goal than maintaining the unity of the expedition.

Queller's book is a thoughtful contribution to the literature on a classic historical problem. His argumentation is even-handed, his control of the sources is impressive, and his conclusions are more plausible than earlier attempts to solve this difficult problem.

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RONALD C. FINUCANE. *Miracles and Pilgrims: Popular Beliefs in Medieval England*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 248. \$13.50.

The demarcation of medieval from modern history reviews, in this journal as elsewhere, has frequently tended to be as much a matter of method as of chronology. Now that the use even of quantitative approaches has ceased to be quite rare in medieval studies, the chronological distinction may seem less necessary; but the world Ronald C. Finucane explores, one in which miracles have become the final credential of sanctity, may nonetheless seem an odd place to employ social analysis and to plot impressive-looking graphs. For it was, perhaps paradoxically, only from the late twelfth century that miracles were regarded as an indispensable qualification for sainthood, whether or not formal canonization subsequently took place. In fact in a number of cases the report of miracles seems to have suggested sanctity rather than the reverse.

Though there is a slight whiff of Coulton and even Lea about this book, Finucane's primary purpose lies in trying to analyze two phenomena: on the one hand the nature and patterns of miracles

recorded at the shrines of seven English "saints" (William of Norwich, Becket, Godric of Finchale, Frideswide, Wulfstan of Worcester, Simon de Montfort, and Cantilupe, plus, for purposes of comparison, an English saint of largely French interest, Edmund Rich, and one French saint, Louis of Toulouse); and on the other the social classes and geographical origins of the pilgrims who came to their shrines. He then attempts correlations and suggestions about the factors which made certain saints popular, and also about the roles—social as well as religious—their shrines played in medieval society.

To a considerable degree Finucane carries out his purpose admirably, and the book has much to teach, especially to a fairly knowledgeable reader. There seems a curious disparity between the largely specialist nature of the work Finucane has done and his manner of presentation, which tends sometimes toward the artificially interest-catching in sentences like "Life in the streets of medieval Palermo must have been, to say the least, colourful" (p. 139). Since the work's value lies largely in its technical scholarship—the useful maps of the distribution of miracles in relation to shrine centers are a case in point—it is disconcerting that the endnotes are often only barely adequate and sometimes not even that. A common fault is the bunching of references to several different points under the same note. Similarly the captions to the sixteen illustrations are totally unhelpful; for the seven manuscripts pictured, neither the MS number nor folio reference is given. And the reader's confidence is disturbed by such errors as "Gloucester Cathedral" (not a cathedral until 1541), "*hoi poloi*," and two different systems of citation within the same note (p. 231 n. 19).

Though not faultless, Finucane's book is a pioneering work in which the social historian's approach (insofar as for "social" we can fairly read "dealing with actual people") adds a needed dimension to a rather arcane corner of ecclesiastical history.

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CAROLE RAWCLIFFE. *The Staffords, Earls of Stafford and Dukes of Buckingham, 1394–1521*. (Cambridge Studies in Medieval Life and Thought. 3d ser., number 11.) New York: Cambridge University Press. 1978. Pp. xiii, 279. \$22.50.

The Staffords is a monograph about one of the most important noble families in late medieval England. Carole Rawcliffe has made extensive use of the widely scattered but unusually rich Stafford ar-

chives. Because of the nature of the extant manuscripts, the heart of the book is a detailed examination of the Staffords' property, finances, and methods of household and estate management, although the first two chapters contain a chronological account of the family that emphasizes its members' political and public careers. The vagaries of survival have also dictated that the study concentrate on Humphrey and Edward, the first and third dukes of Buckingham; relatively little material from the lifetime of Henry, the second duke, remains. The volume includes five well-organized appendixes that present in summary form the raw material on which much of the book is based. They are an important reference tool for anyone doing further work on the family.

The justification for studying the activities of great magnates as landlords is that their success or failure in this area helped to determine their position in national affairs. On one level, this assumption seems unquestionable. As Rawcliffe's book amply demonstrates, the Staffords simultaneously accumulated property and rose to political eminence. What is not so clear is how these two processes interacted at specific moments. Despite the author's occasional effort to draw the connection (as in her suggestion that the first duke was loyal throughout the 1450s because his precarious economic position made him especially anxious for a reward of confiscated property), there is little sense of an organic relationship between the parts of her book on the Staffords' political careers and the larger sections concerned with their property and roles as landlords.

Rawcliffe continues the revisionist work of the late K. B. Macfarlane in her general presentation of the Staffords as literate, relatively well-educated noblemen who managed their estates and finances with intelligence, care, and a fair degree of success. In the case of Edward, the third duke, at least, this view is wrong. The evidence presented simply does not justify labelling him a "renaissance aristocrat" (p. 103). Nor does it sustain Rawcliffe's conclusion that he increased his income by improving the management of his property in England. In 1520–21, the income from all his English estates except those in York was £1,899, far short of the £3,943 estimated in the valor completed at the end of the latter year. The relatively high level of arrears, the numerous vacant tenancies, and the rundown condition of much of the duke's property all refute Rawcliffe's interpretation. Nor did Edward keep his expenditures within reasonable limits as she suggests: in 1520 he acknowledged debts of £10,535 and sold land worth £4,546.

Despite my disagreement with Rawcliffe at many points, I would still recommend her book to scholars interested in the Staffords and the late

medieval English nobility. The care with which she has presented her evidence and sources makes her book useful even if one disagrees with her interpretation.

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MARIE-HUMBERT VICAIRE and BERNHARD BLUMENKRAZ, editors. *Juifs et judaïsme de Languedoc: XIII^e siècle-début XIV^e siècle*. (Collection Franco-Judaïque, number 6.) Toulouse: Edouard Privat. 1977. Pp. 422.

Each year at Fanjeaux, midway between Toulouse and Narbonne, a scholarly congress is held which is dedicated to the religious history of Languedoc in the thirteenth and early fourteenth centuries. The gathering in 1976 dealt with Jews and Judaism. Eighteen papers presented at this meeting by scholars from France, the U.S., Israel, Switzerland, Canada, and Italy have been published in the volume under review here.

Often such collaborative works treat general themes so broad as to give little coherence to the papers presented. The orientation toward religious history has led in this case, however, to a majority of contributions that might be characterized as intellectual history or the history of ideas. With regard to the Jewish community the most important religious issue to emerge from this collection of studies is the reception of the work of Maimonides in Languedoc. Charles Touati provides a clear and useful introduction to the conflicts between the anti-Maimonideans and the pro-Maimonideans with special focus upon the controversy of 1303-06. B. Z. Benedikt attempts unsuccessfully to argue that Manahem ben Salomon ha-Meiri was an original thinker and not simply a follower of Maimonides. I. Twersky, in contrast to Benedikt, cleverly admits the intellectual shortcomings of Joseph ibn Kaspi and recognizes his dependence upon Maimonides, but nevertheless establishes for his subject a prominent place in the history of ideas.

More than a third of the volume deals with religious and socioreligious contacts between Jews and Christians. The dominant theme is that there was greater harmony between Christians and Jews in the south of France than in the north. Gavin Langmuir, faced with the difficult task of explaining why something did not happen, observes that in Languedoc accusations of ritual murder were importations and of no consequence. In the same vein, M.-H. Vicaire points out that anti-Jewish polemic was "bookish" and imported from the north; it lacked a real home in the life of the people of the south. R. Manselli outlines the well-known

process by which attacks on Jews became integrated into polemic against heretics. Sugranyes de Franch's examination of the *Libre del Gentil e los tres savis* demonstrates the highly civilized and sensitive manner in which Raymond Lull treated the debate between Christians and Jews. B. Blumenkranz goes even further in supporting this theme when he concludes that the polemic of Matfre Ermengaud, when examined in light of its accompanying illustrations, indicates sympathy rather than hostility toward the Jews. Pales-Gobilliard's study of the Inquisition follows this same approach.

Of the other studies two deserve special notice. A. Grabois's "Les écoles de Narbonne au XIII^e siècle," is poorly titled, but its examination of the social and political dynamics of the city is noteworthy. Richard Emery's very short study of money lending is furthest from the theme of the conference but is probably the most important piece in the volume. Emery commands for the thirteenth century and even more so for the fourteenth century a vast collection of data that has permitted him to show, for example, that at Perpignan money lending was the essential business activity carried on by Jews. The society seems to have been thoroughly permeated by the use of credit—by 1347, eighty percent of borrowers were *paysans*. Indeed, what today we would call commercial paper was so widely used that it was presented as dowries.

Emery's work, and to some extent Nahon's diffuse article, point to what should be a vital direction of future studies. The available economic data must be gathered and analyzed by modern techniques to ascertain what role, if any, the anti-Jewish policies of the French kings played in bringing about the depression of the later thirteenth and fourteenth centuries. Or to put it another way, were Jewish institutions of credit the margin between success or failure for the French economy at the end of the High Middle Ages?

The remainder of the volume includes workmanlike efforts by Romano on the role played by Jews in the transmission of Arab science, Shatzmiller on scholarly contacts between Jews and Christians at Montpellier, Gilles on Jews in canon law, Dahan on the survival of the work of Jews from Languedoc until the seventeenth century, Dossat on the Jews of Toulouse, and Shahar's speculations concerning the similarities between Jewish and Cathari writings.

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ANGUS MACKAY. *Spain in the Middle Ages: From Frontier to Empire, 1000-1500*. (New Studies in Medieval

History.) New York: St. Martin's Press. 1977. Pp. xii, 245. \$16.95.

Angus MacKay has written, with grace and concision, an excellent study of Spanish history from the eleventh through the fifteenth centuries. The book is divided into two parts extending from about 1000 to 1350 and from 1350 to 1500. In the first part, focusing on the concept of the frontier so much stressed by Claudio Sánchez-Albornoz, he describes the fluid situation of the eleventh century as the caliphate disintegrated into the petty states, whose very lack of unity made it possible for the kings of León-Castile to demand tribute from them (the protection rackets, MacKay calls it). With broad strokes the progress of the reconquest and the concomitant colonization of conquered territory is portrayed. MacKay also assesses the process of acculturation, detailing the work of translation that was to have such great import for Western European intellectual development. Much more difficult to perceive and to depict is the informal acculturation that took place as Christians, Muslims, and Jews rubbed shoulders, sharing and borrowing ideas and techniques. The first part concludes with a discussion of constitutional developments in Castile and the Crown of Aragon. Here I would emphasize that in this period the Castilian monarchy was strong without necessarily being arbitrary. There were acknowledged constraints upon the king, such as the duty to take counsel and to seek consent before levying extraordinary taxes. The king was also expected to abide by the law, to confirm the *fueros*, and to guarantee due process of law. At this time I think these practices were more important than some of the theories expressed in the *Siete Partidas*.

The second part of the book concentrates on the fourteenth and fifteenth centuries, when neither the reconquest nor the frontier were any longer dominant factors in political or social life. During this time of upheaval and civil war, the Trastámara dynasty was established in Castile, Aragon, and Navarre, while economic and social distress was widespread, especially following the Black Death. Apocalyptic and millenarian religious movements and the increasing harassment of the Jews provided outlets for popular frustrations. The trend toward royal absolutism, the problem of the *conversos*, the establishment of the Inquisition, and the influence of humanism are among the principal themes of the closing years of the medieval era.

MacKay has little to say about Portugal and the internal workings of the kingdom of Granada, but he has written a stimulating essay, thoughtful and thought provoking, a welcome addition to the growing body of literature in English dealing with medieval Spanish history. Helpful aids for the stu-

dent include a bibliography of selected works in English and another of books in foreign languages, as well as a chronological table, a list of rulers, a glossary, and an index. There is a graph depicting the devaluation of the Castilian *maravedí* in the fifteenth century; of five maps, the most interesting shows the progress of the pogrom of 1391.

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THOMAS RIIS. *Les institutions politiques centrales du Danemark, 1100-1332*. (Odense University Studies in History and Social Sciences, number 46.) Odense: Odense University Press. 1977. Pp. 397. 90.00 KR.

It is regrettable that international interest in the Scandinavian Middle Ages is limited to the Viking period. Although medieval textbooks in recent years have begun to include complete chapters on Eastern and Central Europe, the Viking outburst into contemporary European society seems to be the only Scandinavian activity worthy of note. Likewise, foreign scholarly contributions to medieval Scandinavia beyond the Viking era are virtually nonexistent. The lively international debate in Old Norse literature and history is proof, however, that neither linguistic barriers nor limited geographic scope are sufficient reasons to explain this lack of interest.

Because linguistic barriers need to be broken to arouse interest in the High Middle Ages it is praiseworthy that Thomas Riis has chosen to publish in admirable French his doctoral dissertation on the royal administration in Denmark. Yet, this is not the book that will break the isolation of Danish medieval scholarship. It is a highly learned work that can only be appreciated by the specialist. The general medievalist will, nonetheless, find it useful, since the author, inspired by the works of Heinrich Fichtenau and Percy Ernst Schramm, shows the growth of a Danish royal ideology as part of a larger European pattern.

The chronological scope encompasses the time between Denmark's emergence from the Viking period (symbolized by the first existing complete charter from 1085) and the interregnum of 1332. The structure and the functions of the institutions that emerged during this period are the focus of the book and are dealt with in three major parts (parts two through four). This presupposes the solution of several textual problems treated in the first part. Important as these issues are for the specialist, they make difficult reading for the uninitiated, and the general reader would be advised to go directly to the second and most important part of the book. Here the author examines in five

chapters the *arengae* of the royal charters, ideology in Saxo, nonwritten sources for royal propaganda (seals, coins, and archeological evidence), royal hagiography, and the actual powers of the king. The most original contribution is the analysis of Saxo, in which Riis examines concepts such as *patria*, *fortuna*, and the principles of succession. He arrives at the conclusion that Saxo, inspired by Archbishop Absalon, developed a highly self-confident royal ideology aimed as much at an international public as at the domestic scene.

A third small part deals with the structure of the political bodies around the king: the *hird* (army), the *meliores regni*, the council, and the *Danehof* (parliament). The fourth part treats the functions of these institutions related to succession, and the legislative, executive, and judicial branches of power. The book ends with a chapter on the Danish administration of Estonia during the period 1238–1346, which provides an area of comparison for the operation of these institutions.

The appearance of a book at this moment on political and administrative history, perhaps the oldest of the historian's endeavors, is justified by the republication (since 1930) of charter material in new critical editions and reflects a re-emergence of political and administrative history in Scandinavia after decades of research devoted to economic and social problems.

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BERENT SCHWINEKÖPER. *Königtum und Städte bis zum Ende des Investiturstreits: Die Politik der Ottonen und Salier gegenüber den werdenden Städten im östlichen Sachsen und in Nordthüringen*. (Vorträge und Forschungen, Sonderband 11.) Sigmaringen: Jan Thorbecke Verlag. 1977. Pp. 167. DM 45.

Beginning with Pirenne's *Medieval Cities*, the study of urban development has become a major area of inquiry in medieval history. Berent Schweinöper's excellent monograph on the policies of the German monarchy vis-à-vis the towns between the Elbe and the Weser in the tenth and eleventh centuries partakes of this vital interest. The author expressed his ideas first in 1969 at a conference on the investiture controversy held by the *Konstanzer Arbeitskreis* for medieval history. Since then Schweinöper has expanded his topic, thereby producing a detailed study of several important German towns.

The towns in eastern Saxony and northern Thuringia have not been investigated thoroughly for the period of the investiture controversy, although Henry IV's wars with his German opponents were largely concentrated in this area. The lack of

sources, mainly about the merchant communities, probably contributed greatly to this, but by examining most of the available evidence, both literary and archeological, Schweinöper has managed to produce a vivid picture of urban life in eastern Saxony.

His book is divided into three chapters. Chapter one begins with a general discussion about the major towns and a brief mention of a dense network of smaller market settlements, which the author cannot discuss individually. The introduction ends with a tripartite division of the major centers into royal towns, episcopal towns, and, finally, one town held by a Saxon noble family. Markets and merchant communities having royal charters of privileges and often dating from the tenth century were found in most of these cities.

In chapter two, Schweinöper discusses thirteen towns, among them Goslar, Magdeburg, Quedlinburg, and Hildesheim. He focuses on their often Carolingian origins, their topography, and the first references to markets and merchant communities, as well as on the troubles with Henry IV during the Saxon revolts.

Schweinöper's conclusions are contained in chapter three. By way of introduction, the author acknowledges his debt to Walter Schlesinger, who suggested that towns in the eastern part of the Empire developed differently from those in the western area. Schweinöper starts with Schlesinger's suggestions, and his own conclusions add substance to these ideas. We are presented with a picture of a growing merchant community, self-contained and self-governing, having its own parish church, and distinguished from the *ministeriales* and *familia* of the king or the bishops. The attitudes of these merchants were very revealing when king and bishop were in conflict. The urban communities were usually reluctant to offend the king, who had granted them protection and privileges, but neither did they wish to be in conflict with the bishops who governed them. Thus, an anti-episcopal communal movement was not in evidence, and antagonism about royal taxes tended to originate not from having to pay them, but from the rudeness and rapaciousness of the royal officers who collected them.

Schweinöper finds that urban life in eastern Saxony in the tenth and eleventh centuries was fairly peaceful and the attitude of the townsmen to the great issues of their day quite neutral. There was neither evidence for conflict with the bishops who headed most of these cities, nor any solid substantiation that the Salian kings, or for that matter, the Ottonians, had a definite prourban program. But Schweinöper's study does much more than produce some clarification about the growth of a communal identity in these towns. He

has also compiled many valuable details about individual towns that specialists in the field of Ottonian and Salian studies will greatly appreciate.

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conti finance makes this collection the essential point of departure for any study of the impact of Visconti financial policies on fourteenth-century Lombardy. Historians of Italian governments will find this collection invaluable.

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CATERINA SANTORO, editor. *La politica finanziaria dei Visconti: Documenti*. Volume 1, *Settembre 1329-agosto 1385*. Preface by GINO BARBIERI. Milan: Giuffrè Editore. 1976. Pp. xxviii, 487. L. 22,000.

Caterina Santoro's intent is to gather together all documents concerning the signorial finance of the Visconti. This first well-edited and attractive volume covers the formative period of the dominion. The editor has collected and arranged chronologically materials drawn from notarial records, local governmental registers, public and private letters, and papal and imperial diplomas. No treasury registers, *estimi*, or *dazi* were included since almost none seem to have survived the wars, insurrections, and natural catastrophes of the past six hundred years. In an effort to be as complete as possible, Santoro has included documents no longer extant but cited, registered, or edited in earlier works on the Visconti. Of the 660 documents included in this collection, 178 have not been previously edited or registered; they were found in the archives and libraries of eight different towns. The largest number (121) came from Voghera. Only 17 are from Milan itself. Santoro introduces the collection with an essay outlining in a general way ordinary and extraordinary sources of income, exemptions offered to individuals and corporations, financial magistracies, and the relationship between the patrimony and state finance.

The collection does not alter radically our understanding of the Visconti dominion. The Visconti lords entered into a series of individual arrangements with various towns and territories; it is difficult to see evidence of a unified fiscal policy. While they usually observed the distinction between the powers and possessions of the *privata persona* and those of the *dominus*, they could still, as in the case of Galeazzo II's marriage arrangements, make private agreements with a significant impact on public finance and politics. Since the collection primarily contains proclamations of taxes (and not receipts) and publications of fines for malfeasance by tax officials (and not court judgments in favor of wronged taxpayers), any suggestion that the collection reflects perfectly the fiscal realities of the Visconti dominion must be tempered. Yet Santoro's inclusion of every document she could find bearing on any aspect of Vis-

ANTAL BARTHA. *Hungarian Society in the 9th and 10th Centuries*. Translated by K. BALÁZS. (Studia Historica Academiae Scientiarum Hungaricae, number 85.) Budapest: Akadémiai Kiadó. 1975. Pp. 147. \$9.00.

The reader's initial reaction to this book is negative for two reasons. First, it is not really a book, but a collection of three extensively footnoted essays: "East Europe in the 9th-10th Centuries," "Hungarian Tribes in the 9th Century," and "From Tribal Confederacy to Statehood." Second, there are quite a few undigested notes, in most instances taken from, or referring to, the works of Soviet historians. Evidently, Antal Bartha, who tries to present early Hungarian history from the eastern, instead of the traditional western, point of view, feels that he has to enrich the knowledge of the reader with new data so that he can base his conclusions upon new information.

Much of the material, however, is not new. In the first essay Bartha deals at length with the Khazar khaganate, within whose socioeconomic structure the early Hungarian tribes lived for some time. They lived in "collectives," he says, and the Khagan, though possessing little power in the Khazar political structure, was held in the highest esteem by all. Military influence rested with the *Bek*, Bartha says, though he fails to define the authority of the *Handur-Khagan*, who, one would presume, held judicial powers. Bartha speaks of Kiev as the economic and marketing center for the Polian tribe, and states that Hungarian warriors served in the Kievan armed forces. This is an important statement; unfortunately, the author fails to cite any references to support it. Neither does he explain why names like Oleg, Olga, and Igor are of Viking origin, while Vladislav is of Russian origin; nor how the fusion between Vikings and Slavs, if there was a fusion at all, had taken place.

The second essay provides a philological analysis of the Turkish and Iranian origins of some Hungarian words denoting food, food production, and animal husbandry. The existence of terms such as "pig breeding" in the primitive Turkish-Hungarian vocabulary shows, Bartha writes, that the Hungarians lived in the northern wooded areas of the parkland steppe, in woods along the rivers.

Terms such as *ur* (lord) and *uruzag* (lordship, country) carry the connotation, he argues, that the area inhabited by these people was lorded over by a local official or potentate. It could also mean, one may add, that the Turkish-Hungarians at this early stage possessed a primitive political and social structure that held them together.

The third essay is the most important and the least original of the three. It comes close to the theories of György Györffy (*Századok*, 1958). Bartha's ideas concerning the cultural and social structure of early tenth-century Hungarian society are based upon archeology and are sound, though occasionally spoiled by generalizations; this, however, may be the fault of the translator. His views on "steppe-aristocracy" provide good reading, but, there again, his appraisal of the confederacy of the Hungarian tribal structure in the tenth century is cut short—perhaps by the translator or the copyeditor.

This book can be an educational experience for anyone interested in that topic. Bartha's attempt to reorient the traditional interpretation of early Hungarian history along eastern lines is not very successful, but he is making a start, the exploitation of which may have an effect upon the new generation of historians in Hungary.

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EVELYNE PATLAGEAN. *Pauvreté économique et pauvreté sociale à Byzance, 4^e–7^e siècles*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques, Civilisations et Sociétés, number 48.) Paris: Mouton. 1977. Pp. xii, 482.

This is a book on poverty as a constituent element of the social and economic structure of the Byzantine empire during the period indicated. It views poverty through consumption, the economic activities of individuals, and the relations which obtained between the poor and other members of society. Such an approach, of course, requires that the character and extent of poverty and the various features associated with it be determined by an analysis of the larger social and economic structure of the society. And this is what Evelyn Patlagean has tried to do. Her book is therefore much more than a description of the condition of poverty in the Byzantine Empire; it is rather an analysis of the social and economic structure of that empire as it relates to the existence of poverty.

The book begins with an analysis of what the texts have to say about the poor and the rich of the Roman world in the classical period. In them the classification of society, though based on wealth,

was expressed in civic terms. With the triumph of Christianity, however, the distinction between classes became purely material in nature. There were the poor on the one hand and the rich on the other, and one of the important functions of the Church was to succor the former. The nature of charity changed accordingly; in contrast to the practice in pagan times, when contributions made by the rich were civic in character, Christian charity developed as welfare designed to ameliorate the material conditions of the poor as individuals. This had the effect of changing the nature of giving and helped to bring about early in the seventh century (during the reign of Heraclius) the elimination of state distributions. The decline of the city—already underway by that time—also helped to discourage older practices since pagan giving, public distributions, and to a considerable extent Christian charity were urban phenomena.

The discussion of living conditions and aspects of the social and economic structure of the empire constitutes the core of the book. Patlagean describes food, housing, disasters, disease, duration of life, marriage (and how it was affected by monasticism), and other aspects of the family. Space is also given to the question of demography. The author accepts the view that the process of depopulation characteristic of the Roman world in the third century came to an end in the course of the fourth, by the end of which an increase in population began that continued down to about 542.

In examining the social and economic structure of the empire Patlagean also discusses the city as it evolved during this period, its economic activities, and the living conditions of its numerous poor; the countryside with its village communities and a peasantry overburdened with obligations and often forced by circumstances, especially in the Balkan peninsula, to flee; the monastery and its role in the social and economic life of the empire; money as the medium of exchange and the role of copper coin in that exchange. The monetary system of the empire was based, of course, on gold, but gold was expensive and as a consequence copper coinage was generally used, particularly by the poor. Patlagean's analysis of prices is the most thorough now available on the subject.

The book is original in that it was composed on the basis of the sources—histories and chronicles, legal and hagiographical texts, inscriptions, some papyri, and archeological finds. The information yielded by these sources is fragmentary, sometimes contradictory, and as a consequence not easy to fit into an integrated whole. This is no doubt the reason why the book is marred here and there by a lack of clarity. It is a difficult book to read, but numerous tables, illustrations, and several maps

help somewhat to elucidate the author's presentation.

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MODERN EUROPE

CHARLES WILSON and GEOFFREY PARKER, editors.
An Introduction to the Sources of European Economic History, 1500-1800. (World Economic History.) Ithaca, N.Y.: Cornell University Press. 1977. Pp. xxx, 256. \$17.50.

This book is a compilation of quantitative materials on early modern European economic and social history. A series of topics was devised, apparently by the editors, Charles Wilson and Geoffrey Parker, and accepted by the other authors. They then sought to find or develop quantitative data suitable to illustrate the topics agreed upon: population, agriculture, industry, trade and transport, currency and finance (some authors also include findings on prices and wages, wealth, and social structure). A total of ninety tables and ninety-nine figures (graphs) are included in the seven chapters. These tables and figures offer population estimates for cities, regions, and societies; baptism, marriage, and burial statistics for particular parishes and towns; agricultural productivity (yield ratios) and profit records for certain estates; local or regional records of growth or decline in particular industries or trades; data on fluctuations in the size of the merchant fleets of certain towns and states; changes in currency values and circulation; estimates of the levels of real wages and the cost of living in certain towns; estimates of personal wealth of social orders or groups; and social structure in certain societies.

The seven chapters deal with Italy (Giuseppe Felloni), Spain and Portugal (Frederic Mauro and Geoffrey Parker), the Low Countries (Jan A. van Houtte and Leon van Buyten), the British Isles (Charles Wilson and Bruce Lenman), France (Michel Morineau), and Germany (Hermann Kellenbenz and assistants). The editors themselves assert that the choice of material for inclusion was "selective, perhaps idiosyncratic," and that the sequence of chapters was designed to reflect movement of the "'core' of the European system" from south to north (pp. xix-xx). A second volume is promised, to deal with "northern, central and eastern Europe," which will supplement and in some degree overlap the contents of this book.

This volume may be used by some readers as a "source"; some professors will probably draw new

lecture material from it, and may use it as a workbook to sharpen the critical expertise of graduate students—it does offer a wide and challenging variety of orderly, quantitative materials for comparative analysis. Many users of the work will want to weigh and evaluate both the provenance and interpretation of data printed here. To help them do so, the authors of all chapters provide references to their sources; some also refer, helpfully, to published criticisms of pertinent manuscripts and monographic literature. Most of the authors explicitly emphasize that their charts and graphs reflect both the uneven character of the surviving data and the limited research so far undertaken in their particular fields. Some authors specifically aim to enjoin other historians to develop additional data for comparative purposes. Ranging in length from only eighteen to as many as forty-five pages, these chapters are fragmented and tantalizingly brief. Each includes a prefatory statement and a conclusion, but most consist of soundings, albeit sometimes deep ones, that simply probe here and there in the panorama of the economic history of a composite and complex society over a span of three centuries. Implicitly they underscore the variety within each European society and the extreme diversity of Europe. Some of these data are stimulating and suggestive. Even specialists in the early modern period can acquire important new insights through this wide-ranging collection.

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J. N. BALL. *Merchants and Merchandise: The Expansion of Trade in Europe, 1500-1630.* New York: St. Martin's Press. 1977. Pp. 226. \$16.95.

The sixteenth century was a turning point in European economic history. A pronounced secular upswing of population, trade, and prices contrasted markedly with West Europe's plodding recovery from the catastrophic fourteenth century. J. N. Ball's survey of commerce in the 1500s describes this upswing in tones of restraint, emphasizing continuity with the past. His theme—never made fully explicit—is that changes in the patterns of trade were slow and complex and were not based upon revolution in technique. Extension of the market and expansion of the scope of mercantile technique was based in part on medieval precedents. Ball regards the influx of New World bullion as an important influence on the changing pattern of trade since new commercial trends appeared first at ports where the supply of new

money entered into the European economy. But the evidence of widespread export of Spanish coin to the Orient is a reminder that Europe's intercontinental balance of trade had not greatly changed since the Middle Ages.

International politics and rivalries play a significant role in Ball's account. Nature's relentless forces, such as climate and the migrations of herring, are very much in the background. Rather, failures of strategy and diplomacy figured importantly in the decline of the Hanseatic League and in the worsening of trade relations between Italy and the Levant at the time of the Turkish conquests. The civil wars in France, along with English competition in the Mediterranean cloth trades, contributed to the end of Lyons and the retardation of Marseilles as centers of international trade, while Habsburg politics and Dutch competition weakened Antwerp. As the century wore on, the future clearly lay with the English and the Dutch.

The institutional foundation for the expansion of sixteenth-century trade was Italian business practices, and Ball gives a good account of the North European adoption of the bill of exchange, the joint-stock company (which had made early appearances in state banks like the Banco di S. Giorgio at Genoa), marine insurance, and state-regulated trade. Other chapters discuss money and banking and the trade in textiles and foodstuffs. By skillful literary masonry, the author builds his chapters from the monographic literature on early modern commerce. One chapter which fails in this respect ("The Merchants") tries in twenty pages to distill and compare the histories of the Fugger, the Ruiz (after Lapeyre), the della Faille of Antwerp (after Brulez), and six other merchant houses—an impossible undertaking which leaves the reader with no clear impression of the distinctiveness of individual methods or fortunes. For the most part, however, *Merchants and Merchandise* is a competent and informative survey.

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LUC MONNIER. *Étude sur les origines de la Guerre de Crimée*. (Travaux d'Histoire Ethico-Politique, number 31.) Geneva: Librairie Droz. 1977. Pp. 146.

A lifetime's professional interest in the ideas and hopes of Louis Napoleon lies behind this thoughtful work on the diplomatic origins of the Crimean War. We may regret that pressing teaching obligations prevented Luc Monnier from giving more time to this topic during his life. He was unable to complete the manuscript of this study but fortu-

nately his widow, Paule Monnier, was able to finish the project. The result is a provocative, interpretive review of a well-worked subject.

The narrative is based, for the most part, on an understanding of the main literature in the field plus some archival research elucidating selected points. There are several citations to a source not properly exploited earlier, the Archives of Prince Napoleon. Although the overall presentation confirms the generally accepted details of the coming of the war, the study makes an important contribution in the freshness of its perspectives and in new evaluations of the roles of major actors who unwittingly led their countries to war.

One factual contribution is of some interest, an appendix discussing the annexation of Cracow by Austria in 1847. The loss of freedom by Cracow is seen in the context of similar developments in Poland and Hungary. That Tsar Nicholas had a hand in all three instances was well known in 1852. At about the time of the Cracow episode, Nicholas lent the government of Louis Philippe fifty million francs, a loan recalled only in 1852 when the government of Louis Napoleon lowered its rate of interest from five percent to four and a half percent. In the correspondence over this issue Monnier found no indication whatever of ill-will between Louis Napoleon and Nicholas, despite the latter's role as a vigilant defender of autocratic regimes. This ties in with one of the book's major concerns, the relationship (one might say, the friendly respect) between Louis Napoleon and Nicholas. The text shows far more concern for this issue than for British policy and the documentation reflects the same—no citations are made from the Public Record Office. While identifying the Cracow annexation as part of the general background, Monnier also gives full attention to the Leiningen Mission in early 1853. Many accounts have neglected this episode and its relation to the Menshikov Mission and thereby have distorted our understanding of that bellicose Russian exercise in diplomatic communication.

The main theme of the study is the path to war of two men who had good intentions, who had respect for one another, and who were both good Europeans. Each was unable to control a situation fraught with uncertainty; Nicholas appears a particularly tragic figure. The most interesting and the freshest observations in the book are, first of all, the reminder that Louis and Nicholas were separated by what amounted to a generation gap in interpreting the meaning for Europe of the 1815 settlement as well as the meaning of 1848 for the system of 1815 and, second, the doubt cast on the extent to which Louis Napoleon had, or could be expected to have had, much interest or stake in the integrity and survival of the Ottoman Empire.

Concern for its survival intact was critical to British entry into the war but hardly influenced the French decision.

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ANN POTTINGER SAAB. *The Origins of the Crimean Alliance*. Charlottesville: University Press of Virginia. 1977. Pp. x, 223. \$17.50.

In his 1956 *AHR* article "A Century of Historiography on the Origins of the Crimean War," Brison D. Gooch observed, "There are no important questions of specific matters of fact yet to be answered." Ann Pottinger Saab sets up Gooch as a straw man to justify still another inquiry into the war's origins, arguing that non-Western materials offer the possibility for new insight. She begins by reviewing a spiral of incidents long recognized as components of the crisis: the Holy Places dispute between Nicholas I and Napoleon III, the Menchikov Mission, Austria's intervention in Montenegro and later ineffectual attempt to become the arbiter of peace. Focusing on events at Constantinople, Saab reassays the contributions of France, Russia, and Austria, but especially Britain and the Ottoman Empire, to the creation of a warlike situation by early 1854. But there she suddenly terminates her narrative, having both justified and accepted as sufficient the standard explanation of the war as simply the product of a diplomatic clash of interests over the Eastern Question.

Even viewed as purely a work of synthesis, the book is overly encumbered with technical deficiencies, including sometimes contradictory analysis, redundant footnotes, and failure to cite original sources or identify quotations when appropriate. Despite the topic's important geographic aspects, no map is provided, and a list of nonstandard abbreviations is also lacking. A far more serious impairment to readability is the author's flagrant misuse of punctuation. Her inconsistency in transliteration will leave nonspecialists struggling with the differences between Reshid Pasha and Neşet Bey, the English corruption "Unkiar Skellesi," and Muslim months spelled in modern Turkish.

Though at her best in explaining the social consequences of economic dislocation in the Ottoman Empire, or when comparing the plenipotentiary powers of European diplomats with the more limited authority of their Turkish counterparts, Saab relies less upon original non-Western sources than upon modern Turkish secondary studies, which in turn lean heavily on Western scholarship. Her argument that the Ottoman statesman Reshid Pasha

personally undermined efforts by Stratford Canning to defuse the diplomatic impasse is buttressed primarily by an apologia of Canning. Therein she misquotes Admiral Adolphus Slade, who attributed responsibility for the naval debacle of Sinope more to the inordinate influence of the misinformed British ambassador than to the imbroglio of Ottoman politics.

In cutting short her narrative Saab exposes a neglect of important secondary studies, such as R. C. Anderson's *Naval Wars in the Levant*, for she fails to grasp the real significance of Sinope as a watershed in nineteenth-century military technology—as important in the war's origins as the Eastern Question itself. Indeed, it was Russian military evacuation of the Rumanian Principalities in July 1854 which altered the entire course of the war and brought it to the shores of the Crimea. Having thus an incomplete understanding of her topic, Saab eschews interpretation with a concluding irrelevancy: "What will happen, in a nuclear age, if the Russian 'Empire' ever breaks up, remains to be seen." Nothing really for Gooch to worry about.

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GERD HARDACH. *The First World War, 1914–1918*. (History of the World Economy in the Twentieth Century, number 2.) Berkeley and Los Angeles: University of California Press. 1977. Pp. xvi, 328. \$12.95.

Researchers in all of the social sciences will welcome this meticulously prepared account of the Great War's direct economic consequences on the leading antagonist nations. By generously translating and supplying his readers with bibliographic and data sources from leading European and North American works on the subject, Gerd Hardach builds an impressive basis for his research. The book is a bit disappointing as an analysis of the war's devastating proportions. One is reminded of Werner Sombart's observation that facts like beads need stringing together. The dust jacket refers to four themes commonly observed about the war: 1.) the decay of the classical world economy based on Great Britain and the gold standard, 2.) the changing geographical patterns of industrialization brought on by the temporary economic paralysis of Europe, 3.) the long-term structural changes in the economies of the warring nations, and 4.) the development of the international labor movement from a low point in August 1914 to the Russian October Revolution in 1917. Only the last of these themes is fully and explicitly examined by the author. He has closely

followed the time constraints observed in the title, and it may be that other authors will offer more substantive conclusions using his sizable empirical base.

The first half of the book is particularly encyclopedic. Hardach has carefully documented the German U-boat campaign, the logistics and effects of the Allies' blockade of the Central Powers, and the general strategies to disrupt overseas trade. He has presented a compilation of food supply aggregates country by country. This format of subdividing chapters by nation is followed throughout the book, making reference particularly easy. The successes and failures in armaments production for each major power reveal much about the genesis of managed capitalism.

In a more analytical mode the second half begins with a chapter on wartime monetary and fiscal policy. All the major powers followed the twentieth-century method of creating money through a central bank to force the populace to save money. In most cases, according to Hardach, these policies fostered significant wealth redistribution within the private sector, particularly enriching producers of war goods. Interestingly, the general population on both sides was led to believe that their soon-to-be-defeated enemies would pay the full costs of the war.

One grasps a better sense of the climacteric from chapter seven. Hardach examines labor aims, organizations, and achievements in each of the countries with an eye toward political realignments after the war. The estrangement of Lenin and the Bolsheviks from the International occurred when more pacifist socialists began to identify with nationalistic aspirations. (Lenin was "pacifist" only in the sense of wanting to abandon the imperialists' wars.)

Hardach illuminates the great powers' war aims in the fading light of nineteenth-century imperialism. According to him, Germany was not led by fanaticism but was clearly the most rationally imperialistic country, achieving some of its objectives both toward its allies and after Russia withdrew from the war. At the very end of the book the legacy of the war is discussed briefly along with changing patterns of world trade and industry. Yet the author relies too much on E. L. Bogart's *Direct and Indirect Costs of the Great World War*. It would be more rewarding to see what conclusions Hardach himself has drawn from the data he has presented.

Notwithstanding the fundamental lack of modeling, testing, or synthesizing from the myriad of facts, this work is scholarly and a sizable contribution to our knowledge of the economics of World War I.

WILLIAM B. HARRISON
Virginia Commonwealth University

CONRAD L. DONAKOWSKI. *A Muse for the Masses: Ritual and Music in an Age of Democratic Revolution, 1770-1870*. Chicago: University of Chicago Press. 1977. Pp. 435. \$22.00.

A lot can be done with the idea that, in one sense or another, the cultural burden of musical life in the nineteenth century was secularized religion. Conrad L. Donakowski has designed a bold but frustratingly diffuse work around that idea with an additional political theme. The most useful aspect of his book is a discussion of how church history related to musical thought, from late eighteenth-century Pietism and liturgical reform to the restoration of plain chant and Gothic revivals, and finally to the Oxford Movement and papal reforms in the middle of the nineteenth century. Donakowski shows how music was central to the refurbishing of religious ceremonies for a more secular and utilitarian age. Hymns and plain chant returned to churches where music was once suspect but was now regarded as a means of inducing morality and piety and sometimes was used to serve the needs of the nation-state. He shows, too, how music acquired a much more central and respected place in learned culture and provided literature with a rich store of images—most significantly "an apt metaphor of community" needed by a society undergoing massive change (p. 310).

But Donakowski tries to do a lot more. He attempts to show how music was used for mass persuasion by a wide range of political and cultural movements—civic cults of the French Revolution, the Saint-Simonians, the nationalism of Verdi, and the democratization of public concerts. Even a discussion of musical culture in nineteenth-century New Orleans is included. He argues that in these contexts communal festivals and sacred traditions were transformed into modern mass events; in revolutionary festivals with large orchestras, he says, "massive music to express mass feelings was becoming normal" (p. 50).

But even though a common political thread may run through his examples, his conceptualization does not go far beyond attributing "mass expression" to them all. The term "the masses" itself has doubtful validity in the early stages of the growth of political participation, and Donakowski takes little heed of the enormous differences among the movements, particularly the social levels of their participants. Some of his examples are indeed spurious; he draws an absurd parallel, for example, between "the musical levée en masse during the French Revolution" and the Handel festivals put on in London in 1784-87 for large but strictly elite audiences (p. 192). He offers a great deal of misinformation and cliché when treating public musical events: he declares without ade-

quate explanation that "the social position of musicians had sunk to the lowest" in the eighteenth century (p. 123); he paints aristocrats as connoisseurs and bourgeois as philistines; and he states erroneously that amateur choruses were drawn from "all classes and conditions of society" (p. 193).

Historical synthesis from such varied sources demands an economy and sharpness of focus absent here. Donakowski displays a rich imagination in linking together different events, but he gets diverted too often into terribly general, and not entirely relevant, questions about the Enlightenment, Romanticism, and theories of culture. His prose is often abstruse—the publisher clearly did little to polish it—and his rhetoric is marred with ahistorical notions about art. Certain errors should also be noted. The 1729 painting of Pannini (p. 156) depicts not an opera but a concert (a cantata presented as a *festa teatrale*). And it is a considerable oversimplification to say, only in passing, that the music of Berlioz was "closer to Palestrina's than Haydn's harmonic methods" (p. 71). Finally, the work in progress of musicologist Ralph P. Locke on the Saint-Simonians shows that the composer Félicien David did not include music written for the Saint-Simonian chapel in his famous symphonic ode *Le Désert* (p. 183) and that Donakowski has mistranslated as "music of the future" David's less Wagnerian statement that God had called him to offer "une musique nouvelle" (p. 181).

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G. R. ELTON. *Reform and Reformation: England, 1509–1558*. (New History of England.) Cambridge, Mass.: Harvard University Press. 1977. Pp. 423. \$15.00.

Reform and Reformation represents G. R. Elton at his stylistic and conceptual best and worst. Uncompromisingly elitist and institutional in his approach, Elton populates Reformation England with inspired managerial types, capable of planning and reshaping society, and meddlesome incompetents, who are either so idiotic they cannot recognize reality or so vainglorious they elect to ignore it. As always Elton is aggressively candid, but for all of his learning, he is strangely moralistic in his interpretation of Tudor England, which seems to be largely an Olympian struggle between good guys and bad guys. Henry VIII is invariably in a "cold fury" or a "mounting fury," never a legitimate fury. Bishop Tunstal possesses a "platitudinous" mind; Sir Thomas More is saintly but

deficient in any "sense of the realities" of Tudor politics; the third Duke of Norfolk is "devious, vengeful, foul-mouthed and essentially second rate" (p. 117); and his ducal brother of Suffolk, according to Elton, should have been flattered to be called "third rate." Even the great cardinal is reluctantly placed among the inconsequential of history, a man whose mind was "fertile but also sterile, mobile but also unsystematic" (p. 83). As for Thomas Cromwell, he was ruthless and ambitious, but his "bloody work" must be balanced against the fact that "the essence of his labours lay in the universal impact of a tenaciously constructive mind upon every problem affecting the body politic" (p. 294).

The key phrase is, of course, a "tenaciously constructive mind," for Elton admires in his hero those qualities which he himself possesses in abundance—a constructive and rigorous mind that delights in planning, rationality, tidiness, and hard work and has little sympathy for, or understanding of, chivalric posing, religious fanaticism, atavistic fears, and military fantasies. Indeed, one of the most refreshing attributes of this book in particular, and of Elton's view of history in general, is that he believes firmly in individual responsibility and motivation; there is no nonsense about his protagonists being the hapless victims of social indoctrination, psychological conditioning, or economic determinism. Elton, however, makes few concessions to misinformation, confusion, sickness, oversight, accident, or irrationality. He suggests that if Wolsey and Henry had had their heads screwed on properly, they would have been able to predict the overwhelming imperial victory at the battle of Pavia (p. 100). Conversely, he confidently states that it was only "when Cromwell entered the lists [against Anne Boleyn] that the inept plotting of the factions suddenly became purposeful" (p. 252). Human folly, greed, half-measures, coincidence, mental aberrations, and the determination to live a fantasy have little legitimate place in the Eltonian scheme of things, except to obstruct and hinder the disciples of rationality and common sense. In a word then, Elton's style of history—daring, provocative, and usually overstated—is very much in evidence: the rich pageantry of a political and intellectual elite making decisions and determining the course of events through their successes and failures.

Although the Eltonian hallmark remains, much has changed. In his preface he speaks of his "younger and more reckless days" and confesses that twenty-five years of scholarship have led him to "alter many parts of the story." One may question whether Elton is in fact any more cautious today than a quarter century ago, and one is sometimes inclined to agree with his own assessment

that "in essentials the forecast written in 1953 differs little from the retrospect concluded in 1976." Nevertheless, Elton has developed over the years. Cromwell and Henry VIII are far less the caricatures they once used to be. The polarity of their personalities has been muted, and the complexities both of motivation and decision making have been vastly increased, making *Reform and Reformation* a far more satisfactory book than *England under the Tudors*. Thomas Cromwell remains the hero, bold, imaginative, forward-looking, and dedicated to a modernity that the actions of irresponsible, narrow-minded, and vindictive men made it impossible to realize. But Cromwell no longer monopolizes the stage, and the king has been firmly reinstated upon his throne; although scarcely an admirable man, he is clearly a force to be reckoned with in the narrative of the Reformation. In fact, the sovereign, for all his neurotic behavior, "brooding egotism," and "penchant for rashness and for crude, obvious and ineffective guile" (p. 116), emerges as the consummate realist who is forever hedging his bets and shying away from extreme methods; it is Cromwell who appears as the idealist who in the end cannot handle reality.

Elton's picture of the court and central government rings true, for he is a master at untangling political plots, both real and imagined, and at tracing parliamentary and diplomatic intrigues. Where the picture becomes less perfect is in its broader outlines—the larger portrait of the body politic that includes the sprawling, squalid city of London, vagabond and husbandman, cutpurse and merchant, priest and parish gentry. All one sees is the institutional head and heart. The failure is particularly evident in the treatment of the Pilgrimage of Grace, Elton's most recent interest. To M. E. James's thesis that the rebellions of 1536–37 were a form of ritualistic dissent staged by the landed gentry, Elton has grafted a factional plot centered at court and designed to oust Cromwell, reverse the king's religious policy, and restore the Aragon succession. The result is to impose on a highly complex and diversified protest, which drew much of its capriciousness from local rivalries and the vagaries of personality, an order and rationality that the evidence does not always support. And, as a consequence, Elton is forced to fall back on idiotic incompetence, largely on the part of Lord Dacry, in order to explain why the conspiracy, if it existed, collapsed so ignominiously.

When Tudor England is viewed so persistently from the pinnacle of the court, one gets a disturbingly elitist picture of events in which the knotted threads of society are woven into a deceptively rational and tidy pattern. History becomes the amusing, if befuddling, record of plots and negotiations that failed for want of their Cromwell and of plans and dreams that prevailed because they

were conceived and executed by the king's great minister. *Reform and Reformation*, however, makes splendid reading and stands as a monument to Elton's immense learning and industry and to the labor of a host of graduate students, who with their master might be described as a *Regnum Eltonianum*.

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EDWARD BRITTON. *The Community of the Vill: A Study in the History of the Family and Village Life in Fourteenth-Century England*. Toronto: Macmillan of Canada, 1977. Pp. xvii, 291. \$22.50.

Edward Britton's *The Community of the Vill* is another welcome addition to the books that have come out of the Regional Data Bank located at the University of Toronto's Pontifical Institute of Medieval Studies. This data bank, which grew out of the research of J. Ambrose Raftis and his doctoral students, contains village reconstitution materials for over fifteen villages that were part of the Ramsey Abbey manorial holdings. The interpretation of the records owes much to the work and writings of its founder so that one might describe the books and articles which have come out of the materials as part of a "Raftis School" for interpreting medieval village society. Britton's book is solidly within this school.

Britton has used the basic social divisions which Raftis found for other villages, that is, there are three main status groups. The dominant group consisted of the A families (sometimes called primary villagers). They were the wealthiest villagers who formed an oligarchy that both preserved its own social status and dominated the other village residents. The A families provided the personnel for village offices such as reeve, hayward, ale tasters, capital pledges, and jurors. The B families (or secondary villagers) were sufficiently well off to support themselves from their land holdings and crafts and to make a bit of a profit as well. Like the A families they protected their family interests for generations, but they seldom held one of the coveted village offices. The C families (or intermediate villagers) did not have large land holdings but instead made their living primarily from wage labor and from crafts. Their families often had some longevity in the villages but none of their ranks could aspire to village governance.

Using these basic groupings, Britton covers such topics as marriage and the domestic group, economic activity in the village, social interactions (including both instances of village cooperation and frictions), demographic trends, and the relationship of the village community to the outside world. He draws his data for the study primarily from the late thirteenth- and early fourteenth-century manorial court rolls of Broughton. While

manorial court rolls are a very rich source for many aspects of village life, they can be extremely quiet about other areas. Only those people who frequently participated in village activities were recorded. Thus one learns a great deal about the A and B families but very little about the C families, women and children, and transients.

In spite of the limitations of the sources and the tedious process of village reconstitution, Britton has reached some interesting conclusions. The A families appear to have preserved their position through a strategy involving both nuclear and extended families. Lands and offices were kept in the family as much as possible, and, to insure legitimacy of offspring, women's behavior was monitored so that those committing adultery or involved in premarital sex were brought into court. In relations between members of the community the A group dominated. When other villagers needed someone to pledge in the court for their good behavior, they most frequently chose their substantial neighbors. Britton sees this as an example of community cooperation, but discusses inter-group conflicts as well. When one looks at assaults and other examples of friction in the community, the most common pattern was fights between the A and the C families. The villagers did not have to remain in the village if there were better opportunities elsewhere or if the community tensions became intolerable. The people of Broughton were very mobile and in the hard years of the early fourteenth century many individuals left the village, particularly members of the C families.

Britton's book is a useful addition to the literature on the medieval villagers, but it is not one of the most exciting contributions. If he had made more comparisons to the findings of other scholars working in the area, including others who have contributed to the Regional Data Bank, the significance of the book and of the village of Broughton would have been enhanced. It remains, therefore, just another village study. The general reader will find the book repetitive and discouraging to read because of the convoluted sentence structure. This is too bad, for the author has made many interesting and original contributions.

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J. A. YELLING. *Common Field and Enclosure in England, 1450-1850*. Hamden, Conn.: Archon Books. 1977. Pp. viii, 255. \$17.50.

Only a generation ago many historians saw enclosure as an institutional device which had the effect of causing—in Maurice Dobb's words—"a rural semi-proletariat to be finally uprooted from the

land and the obstacles to labour mobility from village to town to be removed." For at least three decades this view has been under attack, so much so that little, it would seem, has remained to be said. J. A. Yelling's careful survey of both the historiography and the topography of English enclosures shows that a great deal remains to be discussed. He agrees, indeed, that "enclosure was not the one key step that released commercial or capitalist forces into the agrarian regime." The modest yeoman or peasant, subsisting in a village economy which was yet capable of responding to wider economic forces, might develop his land—given the right geographical situation and soil conditions—while at the same time resisting large-scale enclosure either in a Tudor or in a Georgian context. The pattern and the chronology of enclosure, as is well known, varied greatly from region to region, and the present work is at its strongest in showing the possibilities of regional history, especially in East Worcestershire, where the author has used probate inventories to some effect. His approach is essentially empirical, and at times confusing and over cautious, producing many scores of detailed and sometimes contradictory examples almost for their own sake.

Yet we need these reminders that enclosure was a profoundly complex administrative-social subject. There is much that is healthy in the willingness of the author to eschew the vigorous simplifications of yesteryear and in his cautious attitude to the more recent reactions in the other direction. It is not true, as J. D. Chambers seemed to think, that population increase following enclosure necessarily has any easily proven (or indeed any) relevance to the fact of enclosure in a given parish; nor is it necessarily sensible to suggest, as a protagonist of the New (now rather dog-eared) Economic History has argued, that rent levels tell us much about the economic effects of enclosure. They may do so on occasion; that is all.

Yelling, then, has much that is useful to say. Unfortunately his book is insufficiently astringent, too unwilling to come to any but the most cautious conclusions. Therefore it will not weaken the right-wing dogmatisms that have replaced those of the left—although in providing ammunition for both sides it may ultimately help to enliven a subject that some have thought dead. The debaters of the future will not be helped by a rather poor index.

J. D. MARSHALL
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JOHN MILLER. *James II: A Study in Kingship*. Hove, Sussex: Wayland Publishers. 1977. Pp. ix, 281. £12.50.

This is less a study in kingship than a study of a king. James II is a puzzle now, as he was in his

own time. What made James—loyal, moral, not unintelligent, certainly not unknowledgeable in public affairs, something of a success as Duke of York—a total failure as king? John Miller's King James is different from the duke only in degree. The very loyalty, moral sense, and partial intelligence that made the duke a faithful, if somewhat troublesome, subject to Charles II prompted James as king to expect from his own subjects the same faith, deference, and reverence for authority that he had himself long accorded his brother.

Half of the book has to do with the years before James became king. This is in part a study of James's character and the circumstances that formed it: the defeat and death of his father; the endless squabbling of the court in exile; the young man's hopes for a military, if not a political, career; and, after 1660, the long years of subservience, touched by disappointment and shadowed by the knowledge of what his conversion to Catholicism had already and might yet cost. It is also a study, in detail, of Charles II's rule. Feckless, lazy, easily persuaded, more French than English, Charles repeatedly failed, Miller argues, to seize opportunities to resist the growth of parliamentary power, to exert and strengthen the authority of the crown, to stop the threat of factionalism and political instability.

This view poses difficulties. Perhaps a reassessment of Charles's accomplishments is in order; the idea of a patriot king may well have been taken too far. But Miller's is not so much a reassessment as a rather tendentious rereading of Charles's reign, and a rereading primarily of court politics at that. There was something more to Charles II and certainly more to Restoration politics than what the French ambassador had to say. Then too, perhaps, as Miller suggests, James's reputation has suffered unfairly at the hands of those who overpraise his brother. But to discount Charles's accomplishments is not to enhance James's. King James had the purpose, the application that, in Miller's view, Charles lacked; yet James, not Charles, lost the crown.

Miller explores this puzzle. James was not political, he observes. James understood nothing of compromise, nothing of opinions other than his own, nothing of what his actions might mean to other men. He was a judge neither of men nor of policy. He was first and entirely a Catholic, applying himself singlemindedly to improving the fortunes of English Catholics, trusting to this purpose only those who like him were or professed to be Catholics, ignoring all other considerations and issues in pursuing the one end to which he chose to devote his kingship. Yet, James seems to have had little regard for the pope and less for the conventional attitudes of Catholicism. He was, in Miller's

view, more a kind of puritan than a "conventional" Catholic. This granted, Miller's explanation of James's rule remains unsatisfying. Perhaps, as Miller argues, James did attack the constitution only in what he conceived to be the interests of Catholics. But the king's misjudgment of men and affairs surely had a deeper root. Had he been a thorough Anglican, this ineptness might also have cost him his throne.

Such an analysis might be asking too much. Miller is a historian, not a psychiatrist. He has tried to piece together the picture of a king. The background is at some points superficial, the details inexact; still it is a likeness. He has addressed the necessary questions with intelligence and imagination, even if he has not in all cases answered them. The effort is to be commended.

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C. JOHN SOMMERVILLE. *Popular Religion in Restoration England*. (University of Florida Social Sciences Monograph, number 59.) Gainesville: University Presses of Florida. 1977. Pp. 152. Paper \$4.50.

This book is a study of popular religious works in England during the Restoration era, and its unique quality lies in its thorough knowledge derived from a quantitative content-analysis of more than fifty such works. It may be pointed out, however, that, in spite of the title, it is not a history of "popular religion" in Restoration England—not, at least, in the usual, sociological meaning of the term. Nor, we may add, were the authors included in this study all "popular" religious writers. Among the Anglicans, for instance, four (William Beveridge, Thomas Ken, Simon Patrick, and Jeremy Taylor) were bishops; one (Richard Allestree) was Regius Professor of Divinity and Provost of Eton; and another one (William Sherlock) was Dean of St. Paul's and Master of the Temple. In short, the book deals with popular religious literature, not popular religion as such, in England in the late seventeenth century.

Relying chiefly upon Donald Wing's *Short-Title Catalogue* and the *Term Catalogues* compiled by contemporary London booksellers, C. John Sommerville selects for his study three groups of the most popular religious books, both Anglican and Nonconformist, of the Restoration period: 1.) books which enjoyed unflinching popularity throughout the entire era and had fifteen or more editions, 2.) those which were popular before 1688 and had thirteen or more editions, and 3.) those which were popular in the later part of the period and had seven or more editions. In addition, popular reli-

gious works by other groups such as the Quakers (for example, William Penn and Edward Burrough) and "Liberals" (for example, John Locke and Stephen Nye) are also included for comparison.

The quantitative content-analysis in this study appears to have been a painstaking process. One hundred coding categories are devised with regard to intellectual authority, religious doctrine, and social attitude, and, according to the author, every paragraph in each of the selected works has been subjected to rigorous analysis with its themes carefully coded. Although the conclusions of this study do not radically change the traditional interpretations of religious thought in the Restoration period, they clearly reveal a full, detailed landscape of the popular religious mind in England in the late seventeenth century. The main text of the book is descriptive and elucidating, with the figures of the quantitative content-analysis given in the appendix.

TAI LIU
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THOMAS A. HORNE. *The Social Thought of Bernard Mandeville: Virtue and Commerce in Early Eighteenth-Century England*. New York: Columbia University Press. 1978. Pp. xii, 123. \$12.50.

Thomas A. Horne's admirable study sets the social thought of Bernard Mandeville (1670-1733) in the circumstance of the vast commercial expansion of Holland, his native land, and of England, where he spent more than half his life, practiced medicine, and wrote the works on which his reputation rests. Various influences on his ideas are described: Jansenist appraisal of man's essential depravity; skeptics like Pierre Bayle and his school; the civic humanists' categories of virtue and corruption, and promotion of law enforcement and education. An important stimulus to Mandeville's pen was the optimism of Anthony Ashley Cooper, Third Earl of Shaftesbury, based on a belief in the benignity of nature, in a natural sense of right and wrong, and in an awareness that the individual's welfare was grounded in a public-spirited knowledge that the greatest happiness of the greatest number was a guide to behavior. Like Jacob Viner, and unlike F. B. Kaye, Thomas A. Horne places Mandeville among the forward-looking mercantilists of his time. A discussion of the critics of the work of the author of *The Fable of the Bees* (I and II: 1714, 1729) further illuminates his role in ideological history by noting both refutations and attempts to reconcile contemporary commercial society with those virtues he declared inimical to its prosperity.

Mandeville's conclusions, superficially summarized as "private vice, public benefit," were not integrated by him with conventional morality. The orthodox, like Bishop Edmund Gibson, refuted him by a simple reiteration of Christian teaching. Another of the bench, George Berkeley, accused him of encouraging such evils as robbery and drunkenness. John Thorold declared that man had a sense of duty, denying that industry and contentment were impossible in a wealthy society. Like Thorold, George Bluett conceded that ornaments and improvements might add to the general good, defining vice only as lawbreaking. The Scottish philosophers also redefined vice, and, by substituting utility as a criterion of welfare, reconciled commercialism with republican rectitude. Francis Hutcheson regarded vice only as that which injured another, while David Hume asserted that many of the motives characterized by Mandeville as vicious were not sinful. Time and experience, changing human habits over the years, necessarily affected concepts of virtue. Activity might rouse men from indolence; the provision of luxury might thus strengthen rather than enfeeble the state; the desire for greater wealth could be a spur to progress.

Fears of the fatal debilitation wrought in state and citizen by corruption and luxury largely dominated the pronouncements of moralists of the early eighteenth century. Mandeville, less optimistic about the reform of mankind, examined ways in which his essential self-interest might be harnessed to the needs of a commercial society and the wealth of nations. He shocked many, but obliged, even by often outrageous generalizations, a reappraisal of human psychology in relation to economic activity. Mandeville combined contemporary attitudes to class, the balance of trade, and the power of the state with a more modern approach to wealth and the urban communities developing around him.

This is a well-written volume and provides a first-rate introduction to the social thought of its subject and of the period in which he wrote.

CAROLINE ROBBINS
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JOHN MONEY. *Experience and Identity: Birmingham and the West Midlands, 1760-1800*. Montreal: McGill-Queen's University Press. 1977. Pp. 312. \$18.00.

How did eighteenth-century English communities, undergoing industrialization and rapid population growth, make the social and political adjustments required by their new circumstances? This excellent study describes the processes of these adjustments from the perspective of the people who

were themselves involved, as they responded to immediate issues of the day. Many of these people believed, until quite late in the century, that the population of England was declining and that the growth of manufacturing towns was a distortion of the natural course of social development. Their efforts were, therefore, aimed at providing a continuity between older social and political structures and those which gradually replaced them. It is a particular virtue of sophisticated "local" histories, such as this one, that we are allowed to see these efforts in context, without the distortion of historical hindsight.

Birmingham and the West Midlands cannot, according to John Money, be credited with the kind of dramatic political and social reactions which characterized Middlesex or Yorkshire, for example. (Although there was the "incident" of the famous Birmingham Church-and-King riots of 1791, for which Money's analysis is the most balanced and informative that I have yet seen.) Instead there was a steady accumulation of smaller reactions, whose relationships to each other and to the general course of events must be traced as much through the internal minutiae of local affairs as through their immediate repercussions on a more extensive scale (p. 276). Happily for the reader, the materials for such an investigation were available, and Money has used them with considerable skill. From records as various as circulating library and debating club memberships, acquisitions, and activities; local newspapers and book publishing; political correspondence; and Sunday-school memberships and curricula, we are shown the developing regional self-awareness, the broadening articulacy of the population, and the gradual structuring of "Birmingham interest" as a political force. The chapter, "Taverns, Coffee Houses and Clubs; Local Politics and Popular Articulacy" stands out as a particularly fine example of the author's methods, but the detail of most chapters is very satisfying.

Through the use of local records we see, for example, the beginnings of social linkages between industrial leaders and county nobility and gentry in the establishment of the Birmingham general hospital. Agitation for turnpikes and canal connections show both the unifying tendencies of regional identity vis-à-vis the larger community and the tensions of conflicting interests of farmers and manufacturers. The rise and collapse of the General Chamber of Manufacturers made known to its organizers, and thence to us, the particular concerns of manufacturing regions. And the accounts of parliamentary elections, starting with that of 1774, illustrate a growing comprehensive conception of politics manifest first in the voting of Birmingham residents who were freemen of counties

neighboring Warwickshire, then the political strength of Hemlingford Hundreds was exerted in Warwickshire, until, by 1785, the major manufacturing interests of Birmingham had won the substance of parliamentary representation and were scarcely concerned with the theory.

Money concludes that, in Birmingham, strong elements of conservatism and practical individualism modified the potential radicalism of the artisan population and permitted the retention of the traditional metaphors of social and political hierarchy, while the reality was subtly modified (p. 259). The result was a continuous reconciliation of traditional leadership by county gentry with the exercise of political power by leaders of the new industries. Money's book represents, therefore, a re-revisionist position on the state of British politics in the later years of George III, but with a new and sensitive awareness of the multiple, interacting, and sometimes conflicting principles being represented.

ROBERT E. SCHOFIELD
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PETER ALLAN DALE. *The Victorian Critic and the Idea of History: Carlyle, Arnold, and Pater*. Cambridge, Mass.: Harvard University Press. 1977. Pp. 295. \$13.50.

Peter Allan Dale's excellent discussion of the nineteenth-century idea of history is, in a way, an instance of the historicism it describes. Dale sees his primary subjects—Carlyle, Matthew Arnold, and Pater—as sharing a preoccupation with "the historical process and its philosophical meanings." But his treatment of the development and application of their ideas also exemplifies an historicist position. Seeking alternatives to the certitudes of religion, they found history, as Dale shows, a means to truth. But Carlyle's history, "philosophy teaching by experience," transforms under the pressure of history into what Dale calls Pater's "complete historicist" position, in which history is not a means to truth previously sought through metaphysics, but in itself all the truth there is.

This book is, then, a chapter in the history of ideas. The focus on historicism allows Dale to detect previously unnoticed connections among these writers, and to read his subjects with a fresh and convincing authority. He does not so much alter traditional views of these writers as allow us to see their special qualities in new contexts. And if the whole argument, like much work in the history of ideas, tends to attenuate history into thought, that attenuation reflects quite accurately the direction of the developments Dale is tracing.

Dale shows, for example, that Arnold's ideal of

"perfection" is a secular development of Vico's "*compiuta umanità*." Both ideas aim at values beyond history and beyond the apparently inevitable relativism and fatalism that complete historicism implies. Although history, as Carlyle kept asserting, *Immer wird, nie ist*, he preserved himself from relativism (to which he came very close, in ways that Dale underestimates), by seeing history as the clothing of Eternal Spirit. Arnold, unable to believe in Carlyle's "Eternity," seeks permanence in classical and poetic contemplation that might allow us to learn history's laws and thus to control it, at least a little.

Pater is the climax of Dale's story. Perhaps excessively proud of his discovery of Pater's connection with Hegel and the positivists—the connections have been clear for a long time—Dale is yet very good at showing how positivist ideas mix with romantic attitudes to create Pater's characteristically solipsistic and melancholy historicism. Complete historicism here leads (where all modern avant-garde criticism seems to lead) to the discovery that history can only be about itself, rather than about what history purports to describe. In complex and convincing ways Dale makes a direct connection between historicism and estheticism.

Learned and beautifully argued, Dale's book will be of great value to intellectuals and literary historians. But that is its limitation: by calling the development it describes historicist, and arguing without much reference to any history but the history of ideas, it forces the conclusion at which it arrives. A "complete empiricism," however, might lead to the same place; and in fact, it is difficult by the end to distinguish empiricism from Dale's historicism. In the name of science, Pater is cut off from any truth but the truth of his own sensations; the secular alternative to religion becomes the esthetic, or the human idea of experience. The "realities" of politics, economics, and society can never impinge on such science. Ironically, as Foucault has argued long after Pater, human experience is one subject that cannot, by its nature, become a science.

GEORGE LEVINE
Rutgers University,
New Brunswick

THOMAS BABINGTON MACAULAY. *Napoleon and the Restoration of the Bourbons*. Edited by JOSEPH HAMBURGER. New York: Columbia University Press. 1977. Pp. ix, 117. \$10.95.

Joseph Hamburger has performed a useful service in publishing this handsome little book. Half of it

comprises the lost fragment of Macaulay's projected history of France from 1814 to 1830, the other half comprises the editor's detailed commentary on it. Macaulay, inspired by the July Revolution, wrote the fragment in 1830–31, then gave it up. The text, which he intended to become his first book, is shorter than many of his youthful essays but more careful in its judgments. It begins with a backward look at the Revolution—not with 1789 but (as a result of the regrettable loss of six proof pages) with the fall of the Girondins. It then proceeds through unfriendly sketches of the character and government of Napoleon, his later defeats, the circumstances of Louis XVIII's restoration, aspects of the Charter of 1814 and the political forces surrounding it, and finally to the battle of Waterloo. The commentary is interspersed with comparative remarks on England and brightened by Macaulay's incomparable style. Macaulay's familiar nationalism, liberalism, and romanticism are much in evidence. French historians will learn nothing new from the fragment, but they should find instruction and entertainment in Macaulay's imaginative handling of familiar materials.

Hamburger is well qualified to edit the work, and his introduction is painstakingly done. He ably portrays Macaulay in 1830 and his difficulties in writing the projected history, but devotes excessive space to the physical appearance and fate of the lost fragment, an inconsequential tale which sheds little light on anything of importance. Much of the introduction should be read as an extension of Hamburger's *Macaulay and the Whig Tradition* (1976), whose main thesis is that stability was Macaulay's highest political ideal. In this edition as in the earlier book Macaulay's readers are asked to believe that this son of an abolitionist, celebrated as a glorifier of Cromwell and Milton, a Whig M.P. who made his maiden speech in favor of toleration and who, even as he wrote this fragment, was distinguishing himself as a champion of the Reform Bill, really cared for liberty *only* if it contributed to stability, and for history chiefly because it furnished a vehicle for the teaching of a centrist political science, a rather mechanical "defense of the political center" (p. 9). John Clive's discerning literary biography, *Macaulay: The Shaping of the Historian* (1973), remains the surer guide. Certainly Macaulay, like nearly everyone else, feared disorder and valued "trimming," and Hamburger is justified in pointing it out. But to emphasize this to the exclusion of Macaulay's liberalism, rooted in his background as well as in essentially meliorist (not cyclical) historical perspectives, is misleading and surely unnecessary. Hamburger argues his case skillfully and persistently but again takes it too far, tidying Macaulay's complex views

and rich historiography into a simple and perfect balance of his own making. Consequently he undervalues Macaulay's sympathy with French liberals and republicans, fails to seize a crucial distinction between conservative form and liberal substance in Macaulay's praise of the charter, and misreads Macaulay's account of its violation. But Macaulay, like Scripture, can be quoted to any purpose, and it is instructive to witness here a replay of the earlier struggle over his ambiguities.

GERALD NEWMAN
Kent State University

HOCK GUAN TJOA. *George Henry Lewes: A Victorian Mind*. (Harvard Historical Monographs, number 70.) Cambridge: Harvard University Press. 1977. Pp. vii, 172. \$10.00.

In this modest, well-written book, Hock Guan Tjoa gives an exposition of the ideas of G. H. Lewes, the Victorian editor, Comtean philosopher, and man of letters. Lewes is best known as the consort and literary manager of George Eliot, but Tjoa means to treat him as a worthy thinker in his own right. Tjoa correctly insists that to view Lewes mainly as a literary critic, which some recent authors have done, is to give insufficient attention to his deep interest in science and philosophy. Lewes, he says, was a man of letters in the full Victorian sense—a generalist whose role was to mediate between the middle class and the intellectual elite. For this reason, Lewes's work provides a useful avenue into the world of Victorian thought.

Tjoa deliberately forgoes any effort to present a complete biography of Lewes, concentrating instead on Lewes's "main intellectual preoccupations" (p. v) and their literary and philosophical background. Tjoa divides his book into three sections: first, a description of the world of the men of letters, with a concise account of Lewes's rise in it; second, an exposition of Lewes's work as a moralist, with special regard to his social and esthetic criticism; and third, an analysis of Lewes's struggle to achieve a scientific world view. Throughout, the main intent is to give an appreciation of Lewes's ideas, rather than their formation or significance. Tjoa says nothing of George Eliot's influence on Lewes, and little of Lewes's influence on her or anyone else. He treats Lewes as a type—the higher journalist, struggling with the central intellectual and spiritual problems of his age.

Tjoa's strategy of omitting biographical detail results in an occasional vagueness and a lack of vitality, but it enables him to achieve admirable brevity. This is an interesting and enlightening book, whose importance derives less from reveal-

ing new information about Lewes than from providing a general perspective in which Lewes's thought should be interpreted. Lewes is depicted as a man of importance because he wrestled with issues crucial to the Victorians—how, if possible, to have a science of society, how to establish a "scientific" esthetic, and how to derive from positivism a view of the world to substitute for religion. Tjoa's understanding of various lines of Victorian thought is firm, and his appraisals are judicious. Tjoa gives a clear, concise view of the function of the man of letters, and of the way this function shaped the work of a Radical and positivist intellectual. While he might have offered more of the flesh and blood Lewes, the man behind the ideas so smoothly described here, Tjoa has succeeded in showing that Lewes should be viewed as more than a literary amateur or stage manager for George Eliot.

T. W. HEYCK
Northwestern University

ROBERT H. KARGON. *Science in Victorian Manchester: Enterprise and Expertise*. Baltimore: Johns Hopkins University Press. 1977. Pp. xi, 283. \$16.00.

Manchester, according to Asa Briggs, was the "symbol of a new age," the archetype of the vices and virtues of Victorian industrial society. Mancunian science can also be regarded as an archetype; but, rather than as a symbol, Robert H. Kargon has chosen to treat it as a historical laboratory to examine the social evolution of science in nineteenth-century England. It is an excellent choice: Victorian Manchester possessed a scientific community unmatched in size and talent by any English city except London. Its Literary and Philosophical Society was the prototype for provincial scientific societies throughout the country, and its Owens College became the model for regional university colleges and later for regional "red brick" universities. The scientific community in Manchester experienced in microcosm the trends and developments of Victorian science: the transition from amateur to professional scientists; the influx of German scientific ideas and German-trained scientists; the application of scientific expertise to industry and to such civic problems as sanitation and pollution; the emergence of scientific specialists; the development of scientific education; and the rise of university research laboratories.

In order to trace the evolution of the practice of science and to place that practice firmly within its social and cultural context, Kargon has organized his work around a typology based upon five types of scientists forming a roughly chronological sequence. The gentleman-amateur accepted science

as a part of general learning and pursued it as a genteel leisure activity. The devotee was a specialist who made a career of science but looked elsewhere for his livelihood. The civic scientist was a professional, frequently either self-taught in science or with a German university degree, who sought to earn a living by placing his expertise at the service of industry and the community. The academic scientist not only placed his own expertise in the city's service, but helped to create an educational system that would produce both science and scientists to further the city's goals. And finally the success of academic science paved the way for the university scientist, a specialist dedicated to an ideal of research that went far beyond mere regional aspirations.

Such a typology necessarily involves certain simplifications and shifts of emphasis. Discussions of the role of scientific societies and the continuing importance of scientifically trained chemists in industry, for example, fall off sharply as Kargon's emphasis shifts to academic and university science. Nonetheless, he uses his typology effectively. It permits him to sketch in the careers of representative scientists of the second and third rank, men usually overshadowed by their more prominent peers but who helped to change the character of scientific practice by carving professions from the enterprise of science. It also enables him to dig deeply into the social, economic, and cultural contexts of Mancunian science. One could well wish that some of the inevitable gaps had been filled; but, for me at least, Kargon's perceptive qualitative judgments are more satisfying than the skeletal outlines that could be obtained from a more broadly based prosopography or statistical analysis.

Kargon has taken a major step toward narrowing the gulf between history and the history of science. The influence of science in transforming both the quality and the essential character of life during the nineteenth century makes closing that gulf essential. Thus urban, social, and cultural historians as well as historians of science will welcome this informed and carefully researched book. Most of us, no doubt, would have profited had Kargon done more to relate his detailed study of Manchester to the overall development of science in Victorian Britain. But Manchester is sufficiently important in itself, and the importance of the book far outweighs any deficiencies.

JOE D. BURCHFIELD
Northern Illinois University

TRAVIS L. CROSBY. *English Farmers and the Politics of Protection, 1815-1852*. Hassocks, Sussex: Harvester Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. 224. \$13.50.

BOYD HILTON. *Corn, Cash, Commerce: The Economic Policies of the Tory Governments, 1815-1830*. (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. ix, 338. \$19.50.

Both books under review here are important; both began as doctoral dissertations; and both deal with aspects of English agricultural history in the first half of the nineteenth century. Travis L. Crosby tells a story never told before; Boyd Hilton tells an old story in a very new way.

Crosby begins his with the post-1815 agricultural depression and the rise of the first national farmers' movement under the leadership of George Webb Hall, a Bristol solicitor and wool grower. This episode covers roughly the years 1815-23 and saw the emergence of English farmers as a political force with a Westminster lobby of their own, a national network of local organizations, and an effective rallying cry in agricultural protection. A somewhat different tale follows—namely, the part played by farmers as Conservative Party supporters in the 1830s and 1840s, first in toppling the Whigs and bringing Peel's party to power in 1841, second in toppling Peel's own administration when agricultural protection was abandoned in 1846, and last in briefly reviving the protectionist cry once more in the sharp depression of the early 1850s. Crosby argues that these bursts of autonomous farmers' activities—which he studies in selected, mostly southern, counties—were invariably prompted by the downturn of agricultural prices, which when reversed put a damper on political enthusiasm. English farmers were thus not full-time politicians, except at crucial points in the years 1815-52 when they exerted a significant influence on government. As Crosby puts it, farmers shaped "the outcome of elections and the decisions of members of parliament. They provided the real strength of the protectionist party, and thus brought down one of the most celebrated British ministries. These farmers' agitations demonstrate clearly how political influence in nineteenth century England sometimes worked from the bottom upward" (p. 188).

All of this is refreshing and needed saying. There has been perhaps a bit too much said recently about farmers as deferential Englishmen. But what farmers were, so to speak, in the round, what explains their innate political conservatism—such questions Crosby has left for others to answer. Nor has he said very much about those farmers who supported the first Reform Bill rather than opposed it, or about those farmers who were more interested in currency than in corn as an economic panacea. One wonders why Thomas Attwood's ideas took root in certain parts of the English countryside and not in others. Was this merely the

result of personal circumstances, of the quality of local leadership? More, of course, needs to be said about farmers' leadership generally than Crosby has managed to find room for. A comprehensive social study of this rural bourgeoisie—along the lines of conventional studies of Continental peasantries—plainly needs doing. It is a tribute to Crosby's valuable pioneering study that it prompts these reflections and suggests avenues for future research.

Hilton's book deals with the top of society, not the bottom, with the economic policies of Tory decision makers, 1815–30. One might have thought that enough had been written on this subject; one thinks offhand of the works of W. Smart, William M. Acworth, Donald G. Barnes, and C. R. Fay, not to mention the more recent works of F. W. Fetter, J. B. Williams, and Susan Fairlie, to list perhaps the most prominent. On the other hand, Hilton has done what no one else has done before him. In order to explain how Lord Liverpool and his Tory successors sought to work out satisfactory relations between the economy and the state, he has ransacked the political papers of most leading Tories and of some Whigs, and he has brought to the study of these papers not only the patience of the historian but also the analytical power of an economist—one, moreover, who is familiar with both current and classical theory. Still more unusual, his borrowings in the "dismal science" have not stood in the way of his producing an essay at once remarkable for its wit, its originality, and its lucidity.

Hilton has a fondness for irony which he uses to very good effect. Two examples will demonstrate this as well as make clear some of his principal theses. The first has to do with the return to gold in 1819 and the economic interests of English landowners. He observes, "The possessors of landed property might well have wished to retain a system that had inflated the value of their prime asset while it depreciated the value of money. . . . The irony of 1819 is that the one class that had economic cause to reject gold was, at the time and through considerations of status and social stability, its most enthusiastic champion" (pp. 59–60). The second example has to do with the general economic policy of Liverpool, Huskisson, and company. Conventional wisdom has made the Tory leaders into something like heroic pioneers of free trade, harbingers of industrialism and economic growth. But Tory policies were based less on theory than on cautious groping. If in fact they moved slowly toward free trade—notably in the select committee report of 1821—they did so, not as the Victorians intended, to introduce an international millenium of prosperity and peace, but rather to make England strong in war and to stabi-

lize society in the face of the likely ravages of industrial growth. To quote Hilton once more, "if Liverpool's ministry was the first to strive for a coherent theory of economic policy, it was perhaps the last to regard the industrial revolution as a malignant aberration" (p. vii).

DAVID SPRING

Johns Hopkins University

ANNE HUMPHERYS. *Travels into the Poor Man's Country: The Work of Henry Mayhew*. Athens: University of Georgia Press. 1977. Pp. xiv, 240. \$12.50.

For historians of Victorian lower-class life, Henry Mayhew has often seemed like a cultivated rose blooming briefly in a meadow of wild flowers. Almost two generations before Charles Booth and Seebohm Rowntree made a profession of social investigation, his articles in the London *Morning Chronicle* (1849–50) and the serially published *London Labour and the London Poor* (1851–52) provided a direct glimpse of the lives of tailors, shoemakers, dockers, streetsellers, and the like, for which no counterpart exists for decades before or after. By the century's midpoint, Britons were aware of (though unlikely to peruse) the lengthy transcripts accumulated by investigative royal commissions and parliamentary select committees, and some had read the jeremiads of Thomas Carlyle on the selfishness of the rich and the plight of the poor. Yet, among nonfiction writers, only Mayhew seemed to be able to bring the poor to life without sentimentalizing them, as individuals with a personal view of the world rather than as anonymous components of an amorphous collectivity.

Lacking a Victorian *Life and Letters* or appropriate manuscript collections, twentieth-century scholars have found it impossible to construct a full-scale life of Mayhew (1812–87), the quasi-bohemian son of a London solicitor, who helped found *Punch* in 1841 and who wrote or helped write a score of plays, novels, biographies, and travel accounts, as well as innumerable shorter pieces, comic and serious. With the possible exception of *The Criminal Prisons of London* (1862), none of these approached the originality or significance of his work of 1849–52.

In her new study, Anne Humpherys is able to add few biographical details to those accumulated by John L. Bradley in his introduction to *Selections from "London Labour and the London Poor"* (1965) or by E. P. Thompson and Eileen Yeo in their introductions to *The Unknown Mayhew* (1971). Except for the first chapter, *Travels into the Poor Man's Country* is therefore primarily a work of literary criticism in which Humpherys disentangles Mayhew's accomplishments from his intentions and

places him in his appropriate journalistic and literary context in the age of Dickens, Thackeray, Kingsley, and Jerrold. Mayhew's reports, she reminds us, were intended as the building blocks of a sociological edifice that repeatedly escaped his grasp, though he did develop explanations about the origins of wage levels that differed from those of most economists of his day. His success in the creation of a new form of journalistic art, the uninterrupted first-person narrative set in a vivid descriptive and statistical context, she attributes in part to the creative tension between Mayhew's innate empathy with his lower-class subjects and the relative respectability of his middle-class background.

Humpherys is more at home in the world of literature than of sociopolitical history. Otherwise she would not cite Eric Hobsbawm's *Labouring Men* (1964) as the final word on the standard of living controversy or refer so readily to the journalistic beacon of the free trade movement as "the conservative *Economist*" (p. 18). Nor would she write, referring to April 1858, of "Lord Derby, recently in power with Disraeli" (p. 83); Derby was prime minister at the time. She does succeed in elucidating a number of facets of Mayhew's personality and career, but, as she admits in her "Afterward," a sense of mystery still hovers over the question of why a hack journalist should briefly have flowered as one of history's most remarkable social investigators.

WALTER L. ARNSTEIN
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FRANCIS HEARN. *Domination, Legitimation, and Resistance: The Incorporation of the Nineteenth-Century English Working Class*. (Contributions in Labor History, number 3.) Westport: Conn.: Greenwood Press. 1978. Pp. 309. \$17.95.

Francis Hearn, a young sociologist, has made a diligent and conscientious effort to provide a new interpretation of a segment of English social history by using a conceptual framework derived from the critical theory of Marcuse and Habermas. In the course of "a search for the historical roots of one-dimensionality," he sets out to explain how "the English working class lost its rebellious disposition and entered into the institutions of industrial capitalism" (p. 8). Unfortunately, the book suffers from two interlocking weaknesses that compound the difficulties inherent in an undertaking of this sort. It rests exclusively on secondary sources and on an attenuated and formalistic version of critical theory. As a result, the interpretation is doubly insulated from direct contact with historical reality.

According to the author, "social protest on the part of the oppressed was frequent and usually spontaneous in origin" during the second half of the eighteenth century; the first quarter of the nineteenth century brought the development of "the most intense working-class radicalism in English history." In both periods, working-class culture demonstrated a "capacity to formulate critical, transcendent alternatives" (pp. 24-25). During the 1830s and 1840s, deradicalization took place, as evidenced by the failure of Chartism: "Unlike previous movements, where a playful return to the past produced images of the future shared by workers with diverse traditions and backgrounds, Chartism lacked a transcendent vision." Thus, "caught in the emerging one-dimensionality of industrial capitalism," it was penetrated by "instrumental rationality," and "the emergence of transcendent criteria of action was impeded" (p. 187). The next phase in the process of incorporation, in which the labor aristocracy played a prominent part, took place in the 1850s and 1860s.

Putting aside the question of the usefulness of the theoretical apparatus, the notion that Chartism marks the beginning of the decline of working-class radicalism is a bit idiosyncratic. If such a thesis is to be taken seriously, it requires explicit discussion. It cannot be validated simply by reference to critical theory.

The basic flaw in the book springs from the fact that it embodies in extreme form a misconception that is quite widespread, in one form or another, among nonhistorians, the belief that it is possible to describe and explain the past by bringing to bear a body of concepts or principles whose validity has been established in advance. In this instance, Hearn assumes that critical theory provides a means of "conceptualizing the history of mankind" (p. 10). To be sure, history lacks a conceptual and methodological structure of its own; historians draw on the available intellectual capital—including critical theory—in constructing an analysis of the past. But historians cannot treat what they borrow as a body of established truths that may be applied directly. The rich suggestiveness of *One-Dimensional Man* is lost if it is treated as holy writ or if it is flattened out into the concept of "one-dimensionality."

Hearn has attempted to "resolve the tension between the unique and the general" by a strategy that seeks "to see the general as manifested in the unique or the concrete" (p. 4). What this means in practice, however, is the subjection of concrete historical phenomena to total domination by his version of critical theory.

It should be noted that this review presupposes methodological principles that the author considers inapplicable: "As an interpretative study

which aims to organize previously collected data in terms of a new analytical perspective, this study should not be assessed in accordance with the standards of historiography" (p. 5).

TRYGVE R. THOLFSEN
Columbia University,
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C. H. PHILIPS, editor. *The Correspondence of Lord William Cavendish Bentinck, Governor-General of India, 1828-1835*. Volume 1, 1828-1831; volume 2, 1832-1835. New York: Oxford University Press, for the School of Oriental and African Studies, London University. 1977. Pp. xlix, 740; 1483. \$125.00 the set.

"The great want of the Eastern world," Lord Bentinck once remarked, "may be comprehended in a single word 'knowledge.' I look to steam navigation as the great engine of working moral improvement." It was this intuitive, if oddly stated, understanding of the nature of social change in the early nineteenth century that makes Bentinck's correspondence as governor-general of India of peculiar interest. For many of his contemporaries Bentinck was, as his nephew William Greville put it, a man who was "not right headed," who "has committed some great blunder or other in every public situation in which he has been placed." But as this splendid edition of Bentinck's correspondence makes clear, the period during which Bentinck was governor-general of India was the watershed in the British connection with India. Sixty years before Warren Hastings could assume that the Indian territories were "a temporary possession"; sixty years later Lord Curzon could say it would be "treason to our trust" even to contemplate terminating British rule in India.

No one would claim that Bentinck was personally responsible for the wide-ranging administrative changes that transformed the East India Company's ramshackle arrangements into a modern, rational government; but, as C. H. Philips points out in his admirably lucid introduction to the correspondence, he showed "a sponge-like capacity to take up ideas for change from whatever quarter they came." The result was that his influence was crucial in such social innovations as the abolition of *sati*, the adoption of English as the medium of higher education, and the wider employment of Indians in the administrative services. He permitted a degree of freedom for the press that set the standard for British and, until Indira Gandhi's regime, Indian rulers. The first of the great liberal imperialists, he was troubled by the contradictions between the possession of power and his inability to use it for the welfare of the people of India. A detractor, the historian Edward Thornton, once

said that Bentinck's successes were due to his willingness to make "unrestrained sacrifices" through a recognition of the "spirit of the age." Philips's edition of Bentinck's correspondence provides new illumination on the process of how a modern state was created in India in the nineteenth century.

AINSLIE T. EMBREE
Columbia University

H. S. BHATIA, editor. *Military History of British India (1607-1947)*. New Delhi: Deep and Deep Publications; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. xii, 256. \$14.50.

A more appropriate title for this volume would be "The Development of the British Indian Army." H. S. Bhatia begins with the early forces that protected the East India Company's factories, then traces the developments of the three armies of the presidencies, continues with the unified command, and closes with World War II. He also includes the development of the military arsenals and even a review of British military justice. The book is replete with statistics on the strength and composition of the military units and on the command structures and their responsibilities. It even has a list of the rations that were allocated to both the European and the Indian troops. The weakness to this detailed development is the lack of appropriate annotations; often when he cites a book, he omits the page. The statistical table does not indicate if the figures listed are the actual forces in the field or if they are the proposed and projected forces.

This thin volume is a welcome addition to the field of military history and has much material that is difficult to find. For Bhatia, who served with the Indian Army for thirty years, this book is obviously a labor of love; he constantly refers to "our" army. Unfortunately he is overly critical of the early military history of India and dismisses most of it as a result of "the incurable tendency of the Hindus to every kind of extravagance . . ." (p. 61). He has included brief accounts of many of the critical battles of the eighteenth and nineteenth centuries, accounts that often display his bias, which may or may not be accurate historically. He has enriched his book with personal insights, which give the often neglected Indian view of these events; and he also includes a view of the life of the British officers in India.

There are some minor irritations in the volume, such as using two spellings for the same location or battle (i.e., Plassey and Palasi [p. 75]). The book begins with a glossary of terms and the second chapter is a chronology. Nevertheless this work

gives a refreshing insight into a subject that is often so dull.

GEORGE OCHS
University of Wisconsin—
Oshkosh

ALAN RAMSAY SKELLEY. *The Victorian Army at Home: The Recruitment and Terms and Conditions of the British Regular, 1859–1899*. Montreal: McGill-Queen's University Press. 1977. Pp. 366. \$20.00.

Military historians have tended to stress the dramatic aspects of warfare—the evolution of strategy and tactics, the uniqueness of campaigns, the exploits of regiments, and the valor of commanders. Recently, joining the cadre of scholars examining the social structure of European army officer corps in the pre-World War I era, two British researchers, Brian Bond (*The Victorian Army and the Staff College, 1854–1914* [1972]) and Gwyn Harris-Jenkins (*The Army in Victorian Society* [1977]) have analyzed the professionalization of future generals in the late Victorian era. Reinforcing this trend, Alan Ramsay Skelley now probes a related topic—the welfare of British army enlisted men in the late nineteenth century.

The debacle of the Victorian army in the Crimean War led to reforms that benefited the rank and file in areas of health, education, and discipline. The impetus to these improvements came from numerous royal commissions that investigated the deplorable conditions of barracks life. The public learned that soldiers on peacetime duty at home—sheltered, provisioned, and protected by the state—had one of the most dangerous occupations in the British Isles. As a result of such revelations, efforts were made to stem the high incidences of sickness and mortality that prevailed at army garrisons. Thanks to the propagandist activities of Florence Nightingale and Sidney Herbert, and to the work of a dedicated band of army physicians and engineers, a gradual amelioration of the soldier's habitat occurred. Decent latrine facilities were built, barracks were constructed along scientific principles, and the men were provided adequate heat, water, and light. A tighter system of medical examinations was instituted, and yearly statistical data about the physical well-being of the troops were published. Furthermore, new army hospitals were erected with some attention to space and to ventilation, training for hospital attendants was under way, and the army medical department instructed its doctors in military medicine. Complementing these sanitary and hygienic reforms were attempts to improve the diet of the troops, the cooking in the mess halls, and the

dress of the soldiers, and to control the heavy rate of venereal disease at military posts. As the author explains, the health of the army improved significantly by the 1870s.

Related to this betterment of the soldiers' lot were educational reforms. Illiterate troops were provided opportunities for instruction; soldiers' children were schooled on army quarters; and an inspired group of schoolmasters toiled to elevate the minds of future noncommissioned officers. Linked to these efforts were attempts to improve morale by providing post libraries, a better canteen system, regular band concerts, and organized sports activities. A modernization of military discipline also took place—military courts' procedures were revamped, the penalty of branding a man for desertion or bad conduct was abolished, and, by 1900, the barbaric practice of flogging was virtually eliminated in the army. Clearly, the author contends, army and political authorities made impressive efforts to elevate the level of garrison living conditions, and to make the service more attractive to the working classes of an industrial society.

Yet, regardless of these changes, the army had perennial difficulties in attracting sufficient men for its regiments. Army reformers pondered how to better conditions by improving pay, pensions, career opportunities, and length of service, and by overhauling the antiquated recruiting machinery. Even in the high tide of British imperialism, the public viewed soldiering as demeaning.

This work is crisply and coherently written; it is soundly organized, and the author includes innumerable statistical summaries in the text. The documentation of this study is quite impressive, and the copious footnotes attest to Skelley's mastery of the source material. One can fault the writer in only a few sectors. He could have included more information about the progress made in public health, education, and in penology for civilian society in this period. Why soldiering was so unpopular in this golden age of British overseas expansion is a question that Skelley could have probed, even if he had to speculate on the matter from examples in literature. One also wonders why the writer muffles his enlisted men, for rarely do they speak in these pages. Hundreds of them testified quite frankly to scores of army boards and parliamentary commissions; yet rarely in this work do we hear about their hopes, frustrations, or about their patriotism. However, except for these criticisms, and they are minor ones, Skelley has produced a stimulating work that is a commendable blend of history and sociology.

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REGIS A. COURTEMANCHE. *No Need of Glory: The British Navy in American Waters, 1860-1864*. Annapolis: Naval Institute Press. 1977. Pp. xvi, 204.

This short book has a somewhat misleading subtitle. It is a study of the command of Vice Admiral Sir Alexander Milne of the Royal Navy's North American and West Indian station between 1860 and 1864 rather than the activities of that squadron during the Civil War. Having filed that caveat, let me hasten to add that the book is a useful addition to the literature on that conflict's diplomacy.

Milne was an excellent choice for his assignment. An even-tempered, diplomatic commander, he recognized the necessity for a scrupulous neutrality by his subordinates. As a result, there were few incidents involving British and American warships. The potentially most dangerous one, although it did not directly involve Milne's squadron, was the *Trent* Affair. Regis A. Courtemanche effectively demonstrates the problems the incident created for Milne, whose squadron was not sufficient to contend with the Federal Navy, had war come. One of the strengths of the book is that the author does not overplay the *Trent* crisis but places it in a broader perspective. In the larger history of the squadron, that incident was not terribly important.

Courtemanche devotes his best and most valuable chapter to the role of the Royal Navy in the Anglo-French-Spanish intervention in Mexico (1861-62). Equally rewarding are his insights into the personality interplay between Milne and Lord Lyons, the British minister in Washington. Although quite dissimilar, they formed a good team.

Yet, to say the above is not to suggest the book lacks flaws. Courtemanche does not know the United States Navy and this leads to unfortunate errors. He does not differentiate between sail and steam frigates (p. 3); refers to the former transatlantic liner *Vanderbilt* as a yacht (p. 59); incorrectly assigns the construction of the ironclad *Dunderberg* to the New York Navy Yard (p. 122); and credits the *Niagara* with a battery she never mounted (p. 168). These few errors unnecessarily detract from an otherwise well-researched book. There is a more fundamental problem with the account, however. That is the question of whether it really merited publication as a book. Well over a quarter of the text, notably the chapters on administration and weapons, add very little to our knowledge of the Royal Navy during the early 1860s. Might not the historical profession have been better served by the appearance of the more important portions of the study as individual articles?

K. JACK BAUER
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BRIAN BOND. *Liddell Hart: A Study of His Military Thought*. New Brunswick, N.J.: Rutgers University Press. 1977. Pp. x, 289. \$14.95.

Brian Bond became a neighbor of Liddell Hart's when the captain who taught generals moved in next door in Medmenham. Bond, who is best known for his *Victorian Military Campaigns* and his history of the Staff College, was then a student at Oxford. His close association with Liddell Hart gave him invaluable tutelage and access to the massive collection of Liddell Hart papers, now housed in the King's College archives at the University of London. He assisted Liddell Hart with some of his later books. Of the three of us who have written on Liddell Hart, Jay Luvaas had the earliest professional contacts and Brian Bond the closest, while mine was the oldest, my father being his author's agent. Luvaas and I both published over a decade ago when Sir Basil was still alive, and we were largely concerned with his development and influence up to the Second World War.

Captain Sir Basil Liddell Hart, originally plain B. H. L. Hart, was one of two influential pundits of the interwar years who survived well into the sixties and in their later lives enjoyed a revival. The other, also an Englishman, was Major-General J. F. C. Fuller ("Boney" to those who likened him to Napoleon), whose biography by Anthony John Trythall has also just appeared. Both Liddell Hart and Fuller were not taken very seriously in their own country in the twenties and thirties when they were lucky to sell perhaps five hundred copies of one of their books. But they were heeded abroad, in Germany, where Liddell Hart was translated by the chief of staff, and in Russia, where thirty thousand copies of one of Fuller's works were distributed to the Red Army. In England they attracted enemies as crusading, and sometimes meddling, prophets, and so when scapegoats were needed after the debacle of 1940 they were easily spotlighted. Nevertheless, they worked behind the scenes and emerged after the war with enhanced reputations. Liddell Hart was particularly helped in this respect both by the testimonials of the German generals, especially Guderian, and by the revival after 1944 of his *Strategy: The Indirect Approach*, which has been available in paperback editions in the U.S. ever since.

Where Brian Bond's intellectual history really breaks new ground and contributes to the field is in his more detailed information on and interpretation of Liddell Hart since mid-World War II. He makes effective use of the great captain's papers to show the evolution of his ideas, with whom he communicated, and with what results, though naturally this evidence suffers from problems of

selectivity. Bond shows how Liddell Hart reacted to the atom bomb and to the debate over deterrence or defence in the years up to the publication of his book on that theme in 1960, when it was reviewed by presidential candidate John F. Kennedy. And finally he discusses the way the Israelis have studied and used Liddell Hart's indirect approach with such tactical brilliance. This is perhaps the most interesting chapter of the book, for it is rarely that we have such clear-cut evidence of the influence of a living pundit.

Bond's book conveniently enshrines a man of intellectual significance at least equal to that of Freud in the twentieth century, and for that reason it deserves to be read by historians concerned with life-or-death influences in the Western world.

ROBIN HIGHAM
Kansas State University

ROY DOUGLAS. *In the Year of Munich*. New York: St. Martin's Press. 1978. Pp. 155. \$17.95.

The rationale for publishing this book is not compellingly clear. For the most part it simply retells the already very familiar story of British policy making, almost exclusively at the cabinet level, in the months that span the Anschluss and the German absorption of the mainly Czech portions of the state that survived the Munich crisis. This ground has been so well trodden in recent years by a legion of British and foreign historians that Roy Douglas has only a few new, and not in sum very significant, things to relate. Hence, such interest as the book has lies mainly in the way the author limns the salient features of what may be called the "new orthodoxy" in the treatment of British foreign policy in the thirties.

Like many other recent users of the documents made available under the thirty-year rule, Douglas finds great explanatory power in the primary shaping and constraining conditions of the period: 1.) the exceptional instability of the international order after Versailles; 2.) the profound disparity between British commitments and British military resources between the wars; 3.) the essentially sensible, and even honorable, character of Neville Chamberlain's search for a method of "peaceful change" that would induce a new and bearable equilibrium; 4.) the restraining effects on British policy exercised by military opinion as expressed by the chiefs of staff and by attitudes in the Dominions; and 5.) the crucial role played by public opinion as it shifted decisively from an obsession with peace to an acceptance of the inescapability of war (marked, in the cabinet, by the ascendancy of Halifax). In addition, he deals somewhat dis-

missively with the activities of the government's critics, especially Eden, whose aversion to an accommodation with Italy allegedly deprived Chamberlain of his one real chance to keep Mussolini out of Hitler's camp.

Much of this is pretty well canonical by now, but Douglas infuses a modicum of argumentative freshness into the analysis when, with almost Cobdenite zeal, he flatly states that the taproot of the great calamity was the abandonment, even by Great Britain itself, of free trade principles in favor of protection and other, more extreme forms of economic nationalism. Played upon skillfully by the Continental dictators, what was in essence an economic crisis was transmuted into a political one which no merely economic measures could solve. This is an argument worth pondering, but, in its classically liberal disposition to treat economic and political motivations as clearly distinguishable and even autonomous, it is unlikely to command general assent.

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JOSÉ HARRIS. *William Beveridge: A Biography*. New York: Oxford University Press. 1977. Pp. 488. \$22.00.

Beveridge's name is inextricably linked with the creation of the British welfare state, and at the moment the reputation of both is uncertain. His career raises broad questions about the nature of the modernization of social welfare which began in Britain in the decade or so before 1914 and only reached completion after 1945, and though these have been the subject of extensive historical discussion, at least some of them are by no means dead issues. As an administrator or authoritative commentator Beveridge can be and perhaps often has been credited with too much influence. No one, however, exercised a more significant influence on British social policy over a longer period. The famous Beveridge Plan of 1942 came from a man who in the 1930s had been chairman of the Unemployment Insurance Committee, charged with advising the minister of labor on the administration of unemployment insurance, and who between 1908 and 1911 had helped frame the first unemployment insurance scheme and had been primarily responsible for the introduction of a national system of labor exchanges.

This biography is largely concerned with Beveridge's influence on policy, though his private life and personality are adequately described by José Harris. Her masterly exposition of his ideas and proposals, developed in great and at times some-

what relentless detail, rests on an intimate knowledge of Beveridge's voluminous papers and of the departmental files which have recently been opened. After 1919 when Beveridge left the civil service to become director of the London School of Economics, he was at times far from the center of power, and even before this his wartime move into the ministry of food had taken him away from labor organization and national insurance. His life between the wars raises as many questions about academic freedom as about social policy. Nonetheless, it is his practical and theoretical involvement for over forty years with certain aspects of policy and administration which gives Beveridge's career consistency and importance. At many points Harris deepens our understanding, especially of events during the First and Second World Wars. She also describes two general changes in Beveridge's outlook, which she feels conditioned his thinking on particular issues. Toward the end of the First World War the youthful believer in state intervention suffered a loss of faith, but his new belief in free market economics was destroyed during the slump.

By its nature a biography even of a major figure cannot be an ideal way of providing a complete perspective of the policies which created the welfare state. It is impossible, however, to give an account of Beveridge's life without suggesting or implying some views about the general development of modern policy. Beveridge's dislike of social work, for example, seems to be shared by his biographer, who in her chance remarks tends to devalue its role in the process of modernization; and in general perhaps too much emphasis is given to the broad changes in his outlook. In many ways a very conventional person, Beveridge was not an original thinker at an abstract level, and his opinions on state action and the working of the economy mirrored the shifting mood of many of his contemporaries. Their importance was largely ideological; they supplied rhetorical justifications for policy rather than specific solutions to complex problems. The real influence of his thought lay in details of legislation and administration. As Harris notes, labor exchanges between the wars became points of social conflict, but she does not make the connection, which was analyzed by the American observer, E. W. Bakke, between this fact and the particular legal disqualifications from benefit and administrative procedures which had been built into unemployment insurance from the start. It is possible to argue for an account with a different balance, which would give more weight to the influence on the new policies of a desire to impose social discipline, fears of abuse through fraud or malingering, and a concern to maintain differentials between wages, insurance, and public assist-

ance. Nevertheless, whatever view one takes of these questions of balance and emphasis, this biography is so exhaustive and authoritative that it must remain for the foreseeable future the definitive life.

JOHN BROWN
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GERAINT H. JENKINS. *Literature, Religion and Society in Wales, 1660-1730*. (Studies in Welsh History, number 2.) Cardiff: University of Wales Press. 1978. Pp. 351. £9.50.

Historians of Welsh Methodism have explained the movement's origins as either a reaction to an unreformed Anglican Church or a spiritual rebirth after an era of spiritual stagnation and moral decadence. Geraint H. Jenkins of the University College of Wales disputes these interpretations after a study of the religious literature published for Welsh readers between 1660 and 1730. Despite a continuing difficulty with clerical poverty and its concomitant problems of pluralism and non-residence, Welsh congregations were exposed to increasing numbers of sermons, often in the "plain style" favored by Puritans and Latitudinarians. Recognizing that sermons alone were not sufficient to ground believers in Christian principles, reformers launched a conscious effort to provide religious books in Welsh. The number of books increased dramatically from ten in the 1660s to one hundred and eighty in the 1720s. The subject matter was overwhelmingly religious, with the only serious competition coming from almanacs (14.9 percent of the published books between 1660 and 1730). The Welsh Trust, inspired especially by Stephen Hughes, and the SPCK played key roles in this dramatic expansion of the printed word in Welsh, as did the commencement of book publication at Shrewsbury in 1696.

The growing Welsh literature, Jenkins observes, had little literary merit or academic achievement, though this does not negate the religious and social significance of the material. One of the more meritorious publications was the 1678 Welsh Bible, complete with prayer book and apocrypha. Much of the published material consisted of didactic and devotional works (37.6 percent) and religious verse (21.5 percent), including translations of such popular English publications as Lewis Bayly's *Practice of Piety*, Richard Allestree's *Whole Duty of Man*, John Rawlet's *Christian Monitor*, and John Bunyan's *Pilgrim's Progress*. Controversial works were relatively few (6.2 percent).

Jenkins has been able to identify one hundred and forty Welsh authors in this period, approximately two-thirds of whom came from the western

counties of Wales, where Celtic traditions were strongest. Most authors were Anglican (93), though twenty-five were Presbyterians or Congregationalists, eight were Quakers, and six were Baptists. Vocationally, most authors were clergymen (65), although there were eight gentlemen and eight teachers. Forty of the authors were graduates of Oxford and Cambridge.

One of the most crucial questions of this study involves literacy. There were numerous endeavors to reduce illiteracy during this period, though elementary education was largely restricted to children of nobility, gentry, professional men, substantial farmers, merchants, tradesmen, and artisans. Jenkins scrutinized subscription lists to obtain a composite portrait of the reading public. The principal social groups that subscribed regularly to Welsh books were the gentry, clergy, yeomen, and craftsmen. Although most subscribers supported devotional works, there was also interest in antiquarian and linguistic publications.

There is much value in Jenkins's work, although the chapter on "Moral Imperatives," discussing sabbatarianism, drunkenness, swearing, and moral restraints, lacks substance. The analysis of subscription lists ought to have been accompanied by a substantive attempt to assess literacy, of the sort that David Cressy has undertaken for Tudor and Stuart England. Treating Arminianism as a separate sect is inaccurate. The positive links between Old Dissent and Methodism are surprisingly few with respect to both communities and individuals, at least as noted by Jenkins. Ultimately he rests his thesis on the belief that the devotional books published in this period fostered sanctity, individual and collective piety, and a heightened awareness of the certainty of death and judgment, all of which were basic to Methodism. The fostering of such religious tendencies was certainly the intention of the authors, the Welsh Trust, and the SPCK. The fact that the groups which bought the books included substantial farmers and craftsmen, the very groups which provided the core of Methodism, makes Jenkins's thesis worthy of serious consideration.

RICHARD L. GREAVES
Florida State University

BRUCE LENMAN. *An Economic History of Modern Scotland, 1660-1976*. Hamden, Conn.: Archon Books. 1977. Pp. 288. \$15.00.

The idea behind this work is appropriate. General economic histories of Scotland are not common; indeed, no general modern study has been published since R. H. Campbell's *Scotland Since 1707*.

New research and new interest outside Scotland mean there is a need for a discussion, aimed at an intelligent, non-Scottish audience, of the peculiarities of Scottish economic development and the relationship between the Scottish and English economies.

Modern Scottish economic history is, of course, dominated by the process of industrialization. Scotland was, after all, one of the first countries to industrialize. And while in its earlier stages Scottish industrialization was very similar to English, it took a different path later. As Scotland's economy matured in the later nineteenth century, it came to be more narrowly based than England's, relying heavily on cheap fiber production and heavy industry. In the twentieth century this narrow economic base condemned Scotland to suffer greatly from overseas competition and economic decline.

Bruce Lenman's framework is all one could wish for in a general study of this process. His periodization is good. He begins in 1660, early enough to prepare the way for a discussion of the impact of union on the Scots economy. He carries the story through 1976, late enough to draw some conclusions about the impact of North Sea oil. Between the two dates he makes some interesting points. Yet ultimately he neglects too much of importance and is too often trivial and unclear in his argument. And someone's decision to use a short, ten-page bibliography rather than notes limits the value of his book further.

The major problems in dealing with this topic are, as Lenman himself recognizes, that after 1707 Scotland was an appendage of the larger and more advanced English economy and that Scotland itself is a composite of diverse regions. The second, and lesser, problem is dealt with reasonably well. But the discussion of Scotland's relationship with England is handled very poorly. Lenman points out that within the British economy Scotland once enjoyed the comparative advantage of a cheaper and more passive labor force. Otherwise, connections are rarely made. He says almost nothing about capital and labor flows. Surely the point at which the Scots lost control of their own economy is critical to his story. Surely migration is a major factor in Scottish history, even—and perhaps especially—in the twentieth century. And it is distressing to read discussions of early industrialization which are carried on as if English economic history did not exist.

In the end one comes away convinced that the author was unable to identify the major points of interest in his subject. There is too much trivia, too much concentration on the obvious, and too much neglect of British ties for this book to be of value to the general reader. One would do better to study

the appropriate chapter in Lythe and Butt's recent *An Economic History of Scotland, 1100-1939*.

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FERNAND BRAUDEL and ERNEST LABROUSSE, editors. *Histoire économique et sociale de la France*. Volume 1, *De 1450 à 1660*. In two parts. Paris: Presses Universitaires de France. 1977. Pp. 479, xi; 483-1035.

These volumes begin the series in which scholars associated with the *Annales* school lay the foundations of a new history of France. Fernand Braudel's introduction sets forth major questions for early modern historians: Why did France follow a course of national development in which its formidable resources were expended much more upon European territorial aggrandizement than upon maritime expansion and colonial enterprise? Why did French capitalism remain second-rate at best and hence fail to grasp the means of world domination?

The priority given Pierre Chaunu's essay on the state indicates the increasing attention being focused on the history of political structures that made of the state the "first great victor of modernity," a prerequisite of subsequent revolutions and progress. Describing in turn the "judicial state," the "fiscal state," and the "state of offices," Chaunu attempts to measure the fluctuating power of these systems by quantifying (where possible) the evidence on territory controlled, population ruled, and resources mobilized by and for the state's "technostructure." He strongly emphasizes the positive impact of this technostructure upon social and economic systems. While recognizing negative aspects of social change attributable to the state, Chaunu also credits it for much of what beneficial dynamism there was in French society. And while acknowledging ways in which the state inhibited economic development, he underscores its promotion of conditions favoring long-term growth, such as improved security and communication systems. He enthusiastically asserts that the state should be viewed as a phenomenal economic enterprise in itself, the equivalent and then some of sixteenth-century merchant capitalism, surpassing in fiscal grandeur the American ventures of other European powers. He argues that "the state in France is an America," and that since its allotment of resources was consistent with the French "vocation" of controlling the balance of Europe, the failure to sustain overseas projects should not be considered a misdirection of national strength.

Trading, manufacturing, and banking are the main concerns of Richard Gascon's essay on com-

merce and the towns. Covering first the period of generally expansive prosperity from the 1460s into the 1570s, then the decades of retrogression and recoveries, Gascon traces the organization of commercial space into major regional markets attached to Mediterranean, Nordic, and Atlantic systems of capitalism. Much of the essay follows Lyon's rise and decline as an "economic capital" fulfilling various functions for internal markets while maintaining the principal connections to international systems of trade and finance. As the distribution center for the slowly forming national market and as one of the primary banking centers of the West, Lyon stimulated French capitalism. But, Gascon emphasizes, French markets remained fundamentally regional and banking operations were poorly integrated with other economic sectors. He points out a direct relationship between these structural weaknesses and the fact that foreigners who had little interest in developing a national economy controlled the most profitable undertakings. Furthermore, since the Italians, not the Dutch or English, dominated the foremost markets of the kingdom, much of French enterprise was subordinate to an externally oriented capitalism of the past.

Resentment of foreign control intensified mercantilist economic nationalism, upon which Gascon comments. He shows, however, that the French did little to extend activities beyond internal regional markets. Training methods for careers in commerce and industry were inadequate, and even well-established business techniques of the Italians were only slowly adopted. Gascon also takes up the question of "bourgeois treason," the abandonment by prosperous urban families of marketplace careers for noble status. Noting that the prominence of merchants and merchant-bankers continued to be overshadowed by the superior esteem accorded landed nobility, he places bourgeois conduct in an exonerative context of political and social structures that diverted ambitions and capital into offices and land.

Except for a chapter on rural revolts, Emmanuel Le Roy Ladurie's essay on the peasantry is chronologically organized and weighted according to the significance of crises and recoveries of a Malthusian "ecosystem" tending to "re-equilibrate" at much the same levels of population and productivity between the 1320s and the 1720s. Reviewing evidence regionally, he finds losses of a "first order" of magnitude, which reduced population from a "full world" high of seventeen million to a mid-fifteenth century low of ten million, being made good by about 1560. With few exceptions, however, this "rustic Renaissance" was not accompanied by technical innovations that would have led to breakthroughs in productivity; most

growth in production was achieved by expanding application of traditional methods. Levels thus attained by 1560 generally were not surpassed in similar recoveries that followed subsequent crises, the most serious of which involved only "second order" losses (from ten to twenty per cent) such as were suffered during the worst phases of the civil wars, the Thirty Years' War and Fronde, and the last quarter century of the reign of Louis XIV.

Notwithstanding the ecosystem's relative stability, rural society changed considerably. Horizontal space was radically reorganized and vertical relationships associated with distribution and exploitation of land were substantially altered. Emphasizing effects of inheritance customs, Le Roy Ladurie places changing regional patterns of land ownership and tenure within a context of long-term shifts from seigniorial domination of land toward forms of proprietorship that gradually obliterated "feudal society." He examines the reduction in holdings and numbers of yeoman-like *laboureurs* as part of a decline in the standing of middling and lower groups from the late fifteenth century on. Innovative leasing arrangements, higher rents, price inflation, and falling real wages combined with pressures exerted by the state and towns to impoverish most peasants and to concentrate wealth in the hands of new elites whose luxury consumption reinforced archaic aspects of urban production and trade. He insists, however, that polarization did not produce modern class relations; poverty in the midst of prosperity was absorbed by the ecosystem without destroying the traditional cohesion of village communities.

In the concluding essay Michel Morineau attempts to describe abstractly the internal and external workings of the French economy and to relate that abstraction to concrete living conditions. After discussing conjunctural methodology, he comments on production and exchange with emphasis upon the geographic, social, and political determinants of distribution and participation in local, regional, and international market systems. Despite Morineau's particular concern with money and prices, he does not argue that either should be considered the sought-after "g" factor (a single factor commanding conjunctural evolution). On the contrary, he concludes that no such "g" factor is yet identifiable and that use must still be made of an ensemble of problematical indices. Although the amount of monetary stock cannot now be calculated, the volume of gold and silver in circulation was large enough to help draw France away from a model of development similar to that of Eastern Europe and toward the type which was appearing in Italy, the Netherlands, and England. After discussing the difficulties of interpreting quantifiable evidence on population, agricultural

and industrial production, and commercial activity, he argues that prices—especially grain prices—affected economic growth most significantly. In reviewing efforts to account for price fluctuations, he stresses the impact of military operations and harvest conditions. In fact he contends that increasingly frequent crop failures were crucial in converting transitory price rises of the sixteenth and early seventeenth centuries into economic structures that reduced living standards of middling and lower groups and limited economic opportunities in general.

Thus French capitalism was subject not only to the inhibiting structures of state and society but also to deteriorating agricultural and meteorological conditions. Naturally this makes presentation of a precise model of economic activity all the more difficult. Admitting that the reduction of such a model to mathematical language will not be possible for some time, if ever, Morineau sets forth a diagram which at least describes in words and geometric relationships the dynamics of the French economy. This model, together with supporting evidence summarized in the preceding essays, will probably remain the working paradigm for studies in the social and economic history of early modern France for decades.

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ROBIN BRIGGS. *Early Modern France, 1560–1715*. London: Oxford University Press. 1977. Pp. xi, 242. Cloth \$9.95, paper \$4.95.

To appreciate the difficulty of what Robin Briggs has attempted in this book one has only to imagine trying to write a history of the United States in two hundred pages. Does the writer rattle off a series of facts in a new version of the college outline series? Does he "posthole" by going into some depth on a few themes which, admittedly only a selection, tell a great deal about the period. Does he do an interpretive essay, showing an awareness of recent research, but trying to avoid becoming bogged down in detail?

Robin Briggs has come closest to the last of these alternatives. His *Early Modern France* is a retelling of the story in its familiar sequence: wars of religion, Henry IV, Richelieu, the Fronde, Louis XIV. Much of the narrative is not unlike equivalent pages that could be found in any of a dozen history survey textbooks.

Two chapters raise this book beyond the survey level. In "Economy and Society" some of the recent demographic research is drawn upon. Famines and other crises create a somber backdrop to the analysis of landholding, taxation, standard of

living, and social differences. Briggs sees French society as "massively unequal," ten percent or so having regular meals, ninety percent falling below this line. His discussion of the different groups vying for position within the privileged ranks is fairly rough but, in my opinion, generally sound.

The chapter on "Belief and Culture" is in no way an adequate survey of the philosophical currents or cultural achievements of the great century. What it does is to give a pretty good sense of where the Church was, what it was accomplishing, and what it was failing to accomplish. This is perforce a generalized discussion but it seems to do the job.

Those who want a quick introduction to early modern France will find Briggs a useful guide on a preliminary Cook's tour. Upper-division students, graduate students, or anyone who wants to claim more than the tourist's familiarity will find it necessary to move on to other works, for the past thirty years have seen a great outpouring of scholarship. Even for them, as a good guide, Briggs arouses interest and in his bibliography points the way to further reading.

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EDGAR FAURE. *La banqueroute de Law, 17 juillet 1720*. (Trente journées qui ont fait la France, number 15.) Paris: Gallimard. 1977. Pp. 742.

The system of monopolies that the Scottish banker John Law built up in France during the five years following the death of Louis XIV in 1715 has been widely known but badly misunderstood. Edgar Faure is, I believe, the first author to do justice to the subject because he has the rare qualifications of a statesman with many years of experience in politics and public finance. A Paris lawyer since 1929, he has held office in French governments on and off since the Second World War: prime minister, minister of finance, minister of agriculture, minister of education, president of the National Assembly, and so on. He is able to make judgments and to make sense of situations in a way that is most satisfying to the reader because the gulf of two centuries between Faure and Law is in some ways less than the perennial gulf between us ordinary citizens and our governors. As cosmopolitan as an eighteenth-century statesman, Faure is free of Marxist prejudice and free of what he describes as "the anti-English chauvinism so tenacious in our country" (p. 155). He also benefits by the experience of having written many books, including a study of Turgot's ministry (1774-76) in the same publisher's series. For this book he had the advice of the distinguished Belgian historian, Paul Harsin, who contributed a bibliographical

essay, and he drew upon the work of an American, E. J. Hamilton, and a Swiss, Herbert Lüthy (whom he unfortunately persists in calling "Hubert").

Most of the book is devoted to explaining the subtleties of Law's political position and the technicalities of his financial projects, but there is also new information on his early life, how he became a Catholic and a naturalized French subject, and why the regent Duke of Orleans befriended him. Most valuable of all, the effects of the system are here traced throughout France, province by province, as revealed in the scholarship of the past half century, and some interesting conclusions appear. Faure thinks that the economic benefits for the rural many far outweighed the financial harm done to the urban few, who, in any case, exaggerated their losses. The paper currencies in circulation were so widely used to pay off debts that indebtedness in France was generally reduced, especially in the countryside; much real estate changed hands; and the system gave a useful fillip to the French economy. To his many supporting observations on this subject, the author might have added Roger Dion's wine revolution of 1720 when, we are told, the country folk in general took to drinking wine (*Histoire de la vigne et du vin en France* [1959], p. 595).

Much more remains to be said, too, about the year 1720 in the entire Atlantic economy, for in that year Law's system and the British South Sea company both collapsed, but four permanent, prosperous companies for marine insurance were founded, two in London, one in Rotterdam, and another in Hamburg. Furthermore, foreign governments were eager to make financial experiments: the Danes and the Russians both tried to hire John Law after his political fall in France. Too much has been made of Law's failure, Faure tells us. "He was not a charlatan nor even a poet. . . . There was nothing of the swindler about him. . . . Law failed, but the System succeeded" (pp. 194, 522).

This is an excellent study of a difficult subject, there can be no doubt about it, and let us hope it will soon be translated. But as a critic ought to offer some criticism, let me point out that it was written with no understanding of the bureaucratic revolution which separates us from Bourbon France and which makes meaningless anachronisms of terms like "budget," "deficit," or "*les comptes de l'état*," so fearlessly used by Faure. These terms offend scholarly good sense, but there are more to offend good taste. The book is littered with popular tags and titles of our time, such as "*Mort à Venise*," "*Qu'est-ce qui fait courir Sammy?*" (Samuel Bernard, perhaps?), and "*Le facteur sonne toujours deux fois*." May we add, from de la Motte's

fifteenth fable published in 1719, "*Il faut voir les choses en place.*"

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LEE KENNETT. *The French Forces in America, 1780-1783*. Foreword by JACQUES GODECHOT. (Contributions in American History, number 65.) Westport, Conn.: Greenwood Press. Pp. xviii, 188. \$15.95.

Lee Kennett's study is one of those rare books that will serve several audiences well. It satisfies the rigorous criteria of professional scholarship and is based on extensive archival material in America and France. Concise, clear, and written without jargon, it will make an excellent assignment for both graduates and undergraduates studying the American Revolution or eighteenth-century military history.

The book dispels any lingering misconceptions about the French forces in America. Avoiding romantic sentimentality, Kennett sees them not as "*Les hommes de la liberté*" or the "flower" of the French military, but as a representative "cross-section" of the French army and navy of the Old Regime. They were professionals sent to perform a task and they performed it with notable competence. Although the French army "made some attempts to adapt to conditions of warfare in the New World," Rochambeau for the most part expected to fight as he had learned to fight in Europe. Fortunately, the siege of Yorktown presented few new problems. It was a classic, schoolbook siege, not unlike the fourteen others in which Rochambeau had taken part.

How the French solved the difficult problems of logistics provides probably the best example of their professional competence. In a strange country three thousand miles from home, competing with both allies and enemies for scarce resources in a market disrupted by war and controlled by Americans who sometimes made "excessive profits," the French military's biggest challenge was to feed and supply itself without antagonizing the native population. Kennett's treatment of the battle of subsistence, especially in chapter five, is excellent. His details give the problems the texture of reality and demonstrate the significance of the French success in solving them. For the logistical problems were not simply problems of supply and transport, they also presented dangerous opportunities for misunderstanding and conflict between the two allies. The discussion of logistics also explains why Rochambeau concluded that the American war was "going to be an expensive war. . . ." It proved the last in a series of expensive

wars fought by the French monarchs of the Old Regime.

Kennett's study also contains insights of value to the student of social history. His analysis of the character of the expeditionary force—what the officers considered necessary to bring along in their baggage, the gulf between officers and men, the motives of the officers, the origin and recruitment of the soldiers and sailors—illustrates his point that an understanding of the French forces in America helps us understand the Old Regime in Europe, for they represent "the Old Regime in microcosm." And Kennett does not lose sight of the paradox that this microcosm of the Old Regime advanced the cause of revolution in the Western world.

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LYNN AVERY HUNT. *Revolution and Urban Politics in Provincial France: Troyes and Reims, 1786-1790*. Stanford: Stanford University Press. 1978. Pp. viii, 187. \$11.50.

This work attempts to write political history using the techniques perfected by the *Annales* and Marxist schools of French historiography; to a remarkable extent, it succeeds. Those interested in local history or the origins of the Revolution will welcome its appearance. It is based on extensive archival research in Paris and Champagne; and the 146 pages of text are packed with statistical data from *cahiers*, tax rolls, masonic rosters, marital records, and the like.

Lynn Avery Hunt argues that 1789 was the crucial year in the political history of provincial France—a year when the governmental structures of the *ancien régime* disintegrated and new ones emerged in their place. In examining this phenomenon, the author has chosen to focus on Reims and Troyes. These two cities were much alike administratively and socially on the eve of the Revolution. Both were governed by political elites, men of wealth and property who lived in close proximity in the inner city and whose families were bound by marital ties. In each, the working poor were politically impotent, and a broad group of property owners and guild members enjoyed some economic success but no political power. Important differences, however, also existed. Troyes was dependent on the cotton industry and thus was more deeply affected by the textile depression of 1787-89. Its "rulers" were rich merchants and landowners, intent on protecting their property and privileges. Reims had a more diverse and stable economy, and its political leadership included a

higher proportion of clerics and nobles who, ironically enough, proved to be more adept at governing.

As in most metropolitan centers of France, "permanent committees" sprang up in Reims and Troyes during July-August 1789, after the fall of the Bastille. In other respects, however, the cities followed divergent courses. The "permanent committee" at Reims shared power amicably with the old town council; at Troyes the merchants and professional men in the committee, who had emerged as political figures during the elections to the Estates General and who were backed by militant shopkeepers and artisans, seized complete power. The inability of the Troyes committee to prevent bread riots and protect property led to the intervention of royal troops in what the author calls one of the first instances of counterrevolution in the departments. But in both Troyes and Reims, the municipal elections of January-February 1790 marked the triumph of the "revolutionary" party. Because of the intensity of political feelings and the bad taste left by the "counter-revolution," the victory of the new men was more sweeping at Troyes. The city government at Reims continued to include a few individuals who had been prominent in political affairs during the *ancien régime*.

MICHAEL L. KENNEDY
Winthrop College

AGRICOL PERDIGUIER. *Mémoires d'un compagnon*. Introduction by ALAIN FAURE. (Actes et mémoires du peuple.) Paris: François Maspero. 1977. Pp. 419.

During the July Monarchy many middle-class thinkers attempted to formulate social programs that would meet the needs of the working classes (in what was then a nation of peasants). Agricol Perdiguiet was one of the few contributors to this literature who could pass as a worker—and at that his credentials were suspect. He wrote two notable books and several lesser ones, and he may be the only carpenter in history to have had his correspondence published for the benefit of scholars. If a proletarian with a bibliography seems a contradiction in terms, it is perhaps resolved by the knowledge that in his early thirties he used his wife's dowry to establish a rooming house and install himself as a proper Parisian *petit bourgeois*.

Although Perdiguiet was somewhat less than an ideal representative of *la classe la plus nombreuse*, at least his memoirs accurately reflect the ideological complexity and even confusion of the age. Other diagnosticians insisted that France required guaranteed employment, some version of the dole, workers' cooperatives, communal experiments,

and restraints upon competition, among other things. Perdiguiet prescribed a revivification of *compagnonnage*, those peripatetic craft societies whose ancient ways he had followed in his youth and which were in deep, irreversible decay by the Second Republic. His 1852 reminiscences remain an interesting introduction to that eminently mortal institution (even though there is also a great deal in them of the tedious travelogue—"Chalon is a very pretty city, decorated with monuments, public places, and enjoyable promenades").

Perdiguiet wished to depict the *compagnons* as riding the cresting edge of social change; his text reveals that their principle of unity was archaic—dwelling on craft rather than class—and that they were constantly riven by economic rivalry, mounting instances of scabbing, and the debilitating effects of periodic but apparently irrepressible brawling. It is hard to imagine what social evils he supposed would be eradicated by a reaffirmation of the commitment of the *sociétés* to hocus-pocus passwords and arcane ritual or how he expected workers to afford the huge libraries he advised each of them to accumulate. In all, he wished to see the restoration of a relic, the refurbishing of a museum piece, although in this neomedievalism he was scarcely alone.

The present edition of Perdiguiet's work is the only one in print which includes all later emendations by the author. Alain Faure has provided a useful introduction and some aids to further study.

GEORGE FASEL
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WILLIAM R. KEYLOR. *Academy and Community: The Foundation of the French Historical Profession*. Cambridge: Harvard University Press. 1975. Pp. x, 286. \$14.95.

This gracefully written and highly intelligent book is an accomplished example of an increasingly important genre: analysis at the "point of intersection of intellectual history and institutional history" (p. 13). William R. Keylor traces what he takes to be a paradigm shift in French historiography that led to the establishment, after 1870, of a historical discipline founded on the ideal of "positivistic" history. Key figures in the new history were men such as Gabriel Monod, Ernest Lavisse, and Charles Seignobos. The newly dominant consensus on the nature of history was at first reinforced by patriotic commitment and then tested by the felt need for republican solidarity. The latter political impetus brought "scientific" historians into contact with the Durkheim school in sociology—a contact that helped to foster a crisis in the

historical discipline leading to the emergence of a different understanding of scientific history among *Annales* historians.

This bald summary hardly does justice to the rich content of this excellent book, which forms a subtle, "concrete" complement to the more sociological and model-building approach of Terry N. Clark in *Prophets and Patrons*. Without in the least denying Keylor's notable achievement, I would nonetheless like to raise certain questions concerning his understanding of the intersection of intellectual and institutional history.

Keylor presents national politics as an external context shaping the organization of the historical discipline. What he fails to treat explicitly as a problem is the precise manner in which the foundation of history as a professionalized discipline in France was itself related to intellectual and institutional processes of exclusion that were profoundly political in nature. Certain problems and certain historians, in whose works these problems were active, were situated beyond the pale. By failing to place these processes of exclusion at the very intersection of intellectual and institutional history, Keylor courts the danger of repeating in his own analysis the blindnesses and political exclusions of the historians he studies.

Keylor's historians took great pride in believing that they had excluded literary and philosophical concerns from history. Instead of taking this self-interpretation as highly problematic, Keylor himself uses it to assert that a change in paradigm occurred in French historiography. While the manifest exclusion of literary and philosophical concerns from the realm of scientific history did bring certain changes with it, I do not think that Kuhn's notion of a paradigm shift accurately accounts for them. The basic problem is that of the relationship over time of continuity and discontinuity, repetition and change.

Rather than tackle this formidable problem, I shall simply note a few continuities in what seems to be drastic change. The overt rejection of the "literary" coincided with a tacit reliance upon the most traditional forms of chronicle and narrative to organize "facts." This process, so obvious once it is pointed out, was inestimably important. For example, it helps to explain the marked divergence between twentieth-century narrative history and the modern novel with its often radical experiments in narrative form. Similarly, the manifest dismissal of philosophy furthered a reliance upon a whittled-down variant of traditional philosophy—domesticated empiricism—legitimated, as Keylor observes, by often confused methodological maunderings. The lesson here would seem to be that the rather facile renunciation of tradition, accompanied by a largely uninformed affirmation

of scientific method, increases the chances that one's own style of thought will be repossessed by the least probing aspects of tradition itself. The obvious question is whether figures such as Lavissee and even Seignobos, when compared for example to Michelet, really "cut the umbilical cord" that bound history to literature and philosophy (as Keylor claims) or tied it more securely around themselves in relatively unexamined ways. Indeed, a further question is whether the movement from a narrowly conceived empirical and monographic history to a more broadly based, ambitious, and methodologically sophisticated scientific history in the *Annales* school (which understandably constitutes, for Keylor, the high point of modern historiography and the teleological reference point of his own narrative) contributed much to an explication of the relations among history, literature, and philosophy.

On the level of individuals, Keylor too readily relegates to intellectual history "proper" the study of certain figures. As he puts it, "while the select group of historians whose thoughts and activities dominate the following story were neither the finest historical minds nor the authors of the most memorable contributions to historical scholarship of their generation, it was nevertheless through their efforts, rather than through those of their more illustrious colleagues, that the scientific historical tradition was firmly implanted in the French university system. Sorel, Taine, and Fustel de Coulanges, perhaps the three most renowned French historians of the last half of the nineteenth century, did not play prominent roles in the campaign to create an academic historical profession after 1870" (p. 12). I would suggest that at least one reason they did not play such roles was that their works both helped to keep alive questions that contested the newly dominant consensus and indicated ways in which these questions might still be operative, albeit in a submerged or avoided manner, in the writings of professedly "scientific" historians themselves. It is significant that certain embarrassing questions were raised by someone, disdained by established academic historians, whom Keylor does discuss—Henri Berr, "a secondary school instructor who had been trained in the suspect discipline of rhetoric" (p. 132). Exclusion in all its various dimensions is pertinent to systematic inquiry at the intersection of intellectual and institutional history; indeed it is the counterpart of seeming consensus and order.

DOMINICK LACAPRA
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JEAN-JACQUES BECKER. 1914: *Comment les Français sont entrés dans la guerre*. Paris: Presses de la Fon-

dation Nationale des Sciences Politiques. 1977. Pp. 637. 220 fr.

Jean-Jacques Becker's book is a study of French public opinion during spring and summer 1914. His examination begins with the elections of May 1914 and closes with the end of a "war of movement" in the early autumn. Becker's objective is to understand public opinion in this exceptional moment of French history, but his almost day-by-day account of the July crisis also contributes to the historiography of the immediate causes of World War I. Becker's extensive sources include such traditional documentation as newspapers (Parisian and provincial), prefectural reports, and memoirs of politicians, militant syndicalists, writers, and diplomats. But he also draws on reports of bank and corporation councils and, most interestingly, a collection of responses by teachers to a questionnaire on mobilization and popular reactions to the entry into war. He demonstrates clearly the diverse rather than monolithic character of French public opinion and asserts that many traditional assumptions are indeed questionable.

Becker divides his work into five major parts. In part one, he deals with the prewar nationalist revival as well as its converse, antimilitarism and pacifism. He argues that French nationalism in 1914 was neither bellicose nor numerically very significant. Rather the nationalist revival was a minority current limited to elements in the army, the Church, and a small percentage of intellectuals and university students. It was more an urban than a rural phenomenon. Furthermore, neither *revanche* nor recovery of Alsace-Lorraine motivated Frenchmen to favor war in 1914. They were, by and large, pacific but were willing to fight in defense of their country. He concludes that Frenchmen neither lived in permanent fear of war nor regarded it as inevitable.

Becker's second part focuses on the period of the July crisis. Although he acknowledges the complexity of the situation and the heterogeneous character of public opinion, he concludes that surprise was the dominant French reaction: Frenchmen did not expect Franz Ferdinand's assassination to result in general European war. Becker argues that the departure of Poincaré and Viviani on July 15 on a long sea voyage to Russia reflected the lack of anxiety in France. Other matters, notably the trial of Madame Caillaux, drew far more attention than the developing crisis. Although the urban working class was the heart of the antiwar movement, its actions proved relatively ineffectual since its leadership was divided and lacked a coherent strategy for opposing war.

Reactions to the order for mobilization are treated in part three. In contrast to the enthusiasm

normally portrayed, Becker demonstrates that the characteristic reaction was orderliness. People received the news with stupor or calm resignation, departing to do their duty but lacking enthusiasm. The French public accepted the inevitable, believing that France had tried to avoid war and that Germany bore responsibility for aggression.

Becker then turns to the *Union sacrée*, which has come to symbolize French behavior in 1914. The notion has two implications: a common will for national defense and the unanimity of all Frenchmen. The will to defend the nation was indeed almost unopposed. The end to partisan opposition is more questionable. The *Union sacrée* was regarded as a temporary political truce and was tied to the belief in a short war. Differing viewpoints were temporarily silenced but remained in existence, rendering the *Union sacrée* more myth than reality.

Becker looks finally at the "lost illusions" which came to characterize French public opinion in the early weeks of the war. Believing themselves to be victims of aggression, Frenchmen thought the justice of their cause would lead to rapid victory. Military stalemate induced disenchantment and even some questioning of the war's legitimacy. Gradually, anxiety and even profound pessimism replaced initial confidence.

Becker's book is a massive, carefully presented study of a significant problem in French history. It questions such standard assumptions as the strength of the prewar nationalist revival, the significance of leftist antiwar activities, the enthusiastic reception of mobilization, and the unanimity of Frenchmen during the summer of 1914. It is, therefore, an interesting and important contribution to an always fascinating period.

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GUY BOURDÉ. *La défaite du Front populaire*. (Bibliothèque Socialiste.) Paris: François Maspero. 1977. Pp. 359.

When the French *patronat* recovered from the panic which had sent it scurrying to Léon Blum for help amidst the upheaval which accompanied the installation of the first Popular Front government in June 1936, they began to organize the counter-attack which would restore a government sympathetic to their interests by April 1938 and re-establish their predominance in the face of organized labor by December of that year. Guy Bourdé recounts the success of that counterattack in this partisan yet scholarly volume. The political defeat of the Popular Front was made possible, perhaps inevitable, by the international situation—the

Spanish Civil War and the menace of Nazi Germany—which disrupted the parliamentary coalition. The Government of National Defense under Édouard Daladier, which followed Blum's short-lived second cabinet, sought to appease the dictators at Munich, thus adopting the foreign policy of the Right. To solve the economic crisis it also turned from socialist and state welfare measures to more traditional laissez-faire methods embodied in a series of *décrets-lois* drafted by Paul Reynaud.

Both these foreign and domestic programs played a part in provoking a working-class reaction which, after much hesitation, reached a climax in the unsuccessful general strike of November 30, 1938. Bourd  presents the general strike as the result of a long effort to break the rising power of the *Conf d ration G n rale du Travail* and as a major episode in a decade of social conflict (1934–44) that might almost be called a civil war. While agreeing that the Popular Front lacked coherence in its reform program, especially in its efforts to combat the depression, he insists that it was, after all, overthrown by its enemies. Bourd 's amply documented study of the origins, magnitude, character, and reasons for the failure of the general strike forms the central part of this work and constitutes his most valuable contribution to our understanding of the Popular Front era.

Bourd 's tone is almost always objective, but some interpretations—that, for example, of the Communist Party's role—will be controversial. The definitive history of that tense period awaits the opening of the *Archives Nationales* in the 1980s, but Bourd  has made extensive use of recent scholarship (some of which is based on work in departmental archives) and has worked in Daladier's private papers at the *Fondation Nationale des Sciences Politiques* and in the periodical press of the era. The value of this volume is enhanced by numerous appendixes (fifty-five pages), which present material on the general strike and by a partially annotated bibliography.

The war which followed close on the defeat of the Popular Front brought its enemies to power, but it proved a pyrrhic victory, for "the Liberation would make good the promises of the Popular Front" (p. 292).

WILLIAM LOGUE
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RON J. LESTHAEGHE. *The Decline of Belgian Fertility, 1800–1970*. Princeton: Princeton University Press. 1977. Pp. xix, 259, \$16.50.

Recent historical demography, particularly the variety that has centered on European fertility decline, has yielded four major recent studies that

explore the Portuguese, Italian, French, and German experiences. Now, Ron J. Lesthaeghe adds Belgium to the Massimo Livi-Bacci, Etienne Van De Walle, and John E. Knodel studies. In a deft description and analysis, the Free University of Brussels sociologist makes a strong case for a multivariant explanation of the modern Belgian demographic transition. In this process, the previously attractive Hofstee thesis on "proletarianization" proves inadequate, and a new variable that the author calls "secularization" becomes a major focal point. Lesthaeghe moves carefully through all the other possible indicators of fertility reduction in what is unquestionably an impressive presentation and superior achievement.

This is not merely a well-conducted case study of a small nation's fertility behavior changes. The high degree of economic, social, and cultural heterogeneity in Belgium and the diverse fertility and industrialization experiences make it a more instructive example than France or England. With all its internal variations, this microcosm of Europe becomes a most valid laboratory for social history research. Belgium poses unique problems because its rapid socioeconomic development took place much earlier than the conscious fertility regulation in the late nineteenth century. Furthermore, the bicultural, biethnic division of the state produces separate diffusion patterns within the Flemish and Walloon regions, with ethnolinguistic factors contributing to different attitudes toward fertility. Even in the more common variables such as social mobility, education and literacy, income, patterns and degrees of urban growth, and, especially, occupational factors, significant dissimilarities are present in the Lowlands case. Only a comparable examination of Switzerland could be as valuable in the many contrasts within a European culture.

The author isolates two primary elements as crucial. First, the rate of *marital* fertility decline and the index of industrial-urban modernization interacted as forceful agents, and, second, the language factor helped determine responses. The impact of "secularization," breaking away from organized religion, and diminution of participation and involvement in the religious community is traced and linked to fertility through the cultural component. The book's most telling sections are on the role of Catholicism in the continuity and change of existing moral norms. When religion is studied in the Belgian context of the dual communities, the Flanders-Wallonia dichotomy produces striking results. Lesthaeghe asserts that modernization transformed socioeconomic structure and values in similar ways in the Latin and Germanic areas, yet considerable regional variation in marital fertility was found between the tradition-locked

northern Flemings and the more flexible southern Walloons and urban francophone Flemings. The latter group, socially integrated into, and economically intertwined with, the French-speaking Walloons, illustrates decisively the language homogeneity component.

The primary attribute of this study is the author's full knowledge of Belgian economics, politics, and society. This one central distinguishing trait transforms a fine demographic-statistical analysis into a genuinely rich sociopolitical portrait. My only important reservations concern the need to give fuller recognition (they are properly cited) to the contributions of nondemographer J. Stengers, economists B. Slicher van Bath and H. Van der Wee, and even the 1907 contraception ideas of C. Jaquart.

This first-rate integration of political economy and social demography is imaginative, objective, and very wise. The rigor of the statistical probes, mainly in illuminating occupational and language considerations behind falling fertility, is convincing. There will be dissent, mostly from the monocausal camps, but this multivariate approach results in scholarship that is firmly constructed and cannot be ignored.

The publisher has done its usual superior job in presenting a complex array of graphs and charts in a most intelligent and helpful manner.

PIERRE-HENRI LAURENT
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PIETER J. VAN WINTER. *American Finance and Dutch Investment, 1780-1805, with an Epilogue to 1840*. English adaptation by JAMES C. RILEY In two volumes (European Business: Four Centuries of Foreign Expansion.) New York: Arno Press. 1977. Pp. xx, 1171. \$68.00.

The considerable merits of Pieter J. van Winter as a historian were first noted in this journal when Dirk W. Jellema discussed the essays published in honor of his retirement in 1965 from the University of Groningen (*AHR*, 72[1967]: 526-27). That Van Winter's work has remained virtually unknown in the United States is largely because the Dutch language is little understood here; hence Dutch scholarship often does not receive the wide recognition it deserves. The Arno Press has partially remedied this situation by giving us an English edition of the two volumes on the early history of U.S.-Dutch financial relations, a work by which Van Winter first established his reputation some fifty years ago.

The new edition has been thoroughly revised and brought up to date by the author himself, with the assistance of a young American scholar, James

C. Riley of Indiana University. Riley is to be congratulated on having revived and carried out an all-but-abandoned project. On the whole the revision is very successful; it incorporates the results of post-1933 research on both sides of the Atlantic.

I can only wish that Riley's revision had extended to the translation of the Dutch text, which apparently was completed before he took over the project. With the possible exception of chapter eight, the English text does not do justice to the crisp and lively style of the Dutch original; at times the English is obscure or even misleading. At least twice the revised version refers to material deleted from the original (p. 429, note 5; p. 930, note 4). This seems a dubious practice. If the factual material is worth referring to, it should be retained, especially in a study whose continued usefulness lies in its documentation. It seems unfair to refer an interested reader to a foreign language edition that no longer may be easily accessible. It is unfortunate in this connection that the Library of Congress lists the English version under the editor's rather than under the author's name. If the two editions are complementary they should sit side by side on the shelves of libraries that own both of them.

In essence, however, the two versions tell the same story. Van Winter describes in great, perhaps occasionally excessive, detail the financial operations whereby a group of Amsterdam merchant-bankers, from 1782 to 1805, made significant amounts of Dutch capital available for the political and economic development of the infant American republic. The new title appropriately indicates this focus on investment and finance, a focus sharpened in the English edition by combining two of the original Dutch chapters that dealt with commercial relations.

The bulk of the story concerns United States government loans, including a fascinating chapter on Dutch participation in financing the Louisiana Purchase. Three chapters deal with land speculation in upstate New York and elsewhere, supplementing what was previously known about this subject through Paul D. Evans's monograph on the *Holland Land Company* (1924). Chapter twelve discusses Dutch interest in U.S. internal improvements, specifically in canalization of the Connecticut, Potomac, and James Rivers.

Van Winter has told his story not only in terms of impersonal economic forces but also as it developed through the passions and purposes of individual actors. What may seem like a highly technical subject has thereby acquired a broader human interest. Throughout, the narrative has a ring of authenticity, which only the most painstaking, extensive research in Dutch and American archives could have imparted. It is altogether a splendid

book, an appropriate contribution to the bicentennial celebration of United States independence.

BERTUS HARRY WABEKE
Alexandria, Virginia

EMANUEL HALICZ. *Danish Neutrality during the Crimean War (1853-1856): Denmark between the Hammer and the Anvil*. Translated by JANE CAVE. Odense: Odense University Press. 1977. Pp. 247. 70.00 KR.

In this work Emanuel Halicz describes the efforts of Denmark's government to remain neutral during the Crimean War. He devotes his attention primarily to the attitudes adopted by the warring powers. Because the Danish government was conservative and pro-Russian, its neutrality was most detrimental to Russia. Nevertheless, the Russians recognized the necessity of Denmark's position and therefore never demanded its change. France, on the other hand, continuously sought an alliance with Denmark to strengthen the Baltic front against Russia. When the Danes refused, the French pressured them further in 1855 by tempting Prussia with the prospect of eventually acquiring Schleswig-Holstein in return for Prussian support of the Western powers.

Halicz, a Polish historian who emigrated to Denmark in 1973 and now lectures at Odense University, is most skillful in his study of Danish-Prussian relations and of the impact that Denmark's neutrality had upon other minor European states during the war, particularly the Netherlands, which was in much the same position as Denmark. The book does, however, possess some serious shortcomings. Rather than present his own interpretation, the author relies chiefly upon paraphrasing the correspondence of foreign diplomats and members of the Danish government to describe events and policies. He never defines clearly the particular predilections that the A. S. Ørsted-C. A. Bluhme ministry possessed regarding foreign policy. The characteristics of the Danish government led by P. C. Bang and L. N. Scheele in 1855 that made it more pro-Western and less acceptable to the Russian government are equally unclear.

Halicz fails to summarize adequately military activities in the Baltic that had an impact upon Danish foreign policy. It is particularly surprising that he does not consider Swedish foreign policy, since the Danes originally declared neutrality in concert with their Scandinavian neighbor. Nor does he mention frequent Swedish negotiations to obtain an alliance with Britain and France, an interesting contrast to Danish policy. Finally, the important diplomatic mission of the French military hero, General F. C. Canrobert, to both Sweden and Denmark deserves greater attention and clarification than this work provides.

Halicz is correct in his contention that wartime neutrals—including Denmark during the Crimean War—ought to receive greater attention from historians than they often do. The author has partially fulfilled this need but also has left important aspects of his subject unresolved.

LELAND B. SATHER
Weber State College

OLE KRISTIAN GRIMNES. *Hjemmefrontens ledelse* [The Home Front Leadership]. (Samtidshistorisk Forskningsgruppe. Norge og den 2. verdenskrig.) Oslo: Universitetsforlaget. 1977. Pp. 575. 39.00 KR.

The Norwegian resistance movement during World War II emerged as the result of a continuously evolving process that briefly carried over into the postwar period. In this highly specialized study, Ole Kristian Grimnes has focused his attention on the leadership of the resistance or, to use the Norwegian term, the Home Front. The study is original in that it is the first scholarly attempt to provide a full chronology of Home Front leadership from its earliest unsuccessful efforts in 1940-41 through the role that the leadership played in the formation of the postwar coalition government. Equally original is Grimnes's coverage of the three parts of the resistance: *Kretsen* (the Circle), *Koordinasjonskomitéen* (the Coordination Committee), and *Milorg* (the Military Organization). He views these groups as equal partners, each carrying out highly specialized tasks, rather than emphasizing one branch over the other two, as has sometimes happened.

The initial period of the resistance leadership prior to 1943 was a time of trial and error, consolidation, and establishment of specialized functions. From 1943 onward the leadership of the three branches of the Home Front functioned on an equal basis, with lines of communication linking the three and the government-in-exile in London. One of Grimnes's main themes is the growing cooperation and coordination between the Home Front and the government and the increasing respect and authority that the Home Front gained among the populace. For most of the war the Home Front acted on an informal basis, with relations among its branches not specifically spelled out. Only during the last months of the war, in the winter of 1944-45, was a centralized leadership established that incorporated representatives from *Kretsen*, *Koordinasjonskomitéen*, and *Milorg* into a superior Home Front leadership.

The study is thoroughly researched. Grimnes has examined the available documentary material, which at times is quite sparse. He has consulted the tremendous variety of secondary literature on

the subject. Finally, he has interviewed the surviving members of the Home Front leadership.

Grimnes's attention is at all times riveted on the Home Front leadership. He does not attempt to place the subject matter within a broader perspective. Other forces, such as Communist and non-Communist opposition groups within the resistance, appear only within the context of their influence on the leadership. The study is for the specialist, not the general reader.

The content is at times repetitious, indicating that careful editing might have improved the more than five-hundred pages of text. The author's style is occasionally somewhat wooden. Such reservations, however, are minor. Within the boundaries that he has chosen, Grimnes's study of the Home Front leadership is a valuable addition to Norway's occupation history.

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CARL LENNART LUNDQUIST. *Council, King and Estates in Sweden, 1713-1714*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 50.) Stockholm: Almqvist and Wiksell International. 1975. Pp. vi, 215. 47.00 KR.

The topic of this study is a promising one: the attempts of the council and estates, acting during Charles XII's prolonged absence in Turkey and against his express command, to undertake and legitimize peace negotiations with Sweden's enemies in the apparently desperate circumstances of 1713-14, during the Great Northern War. Specifically, the author reappraises the old conspiracy theory originating with the French envoy, Camperdon. According to this theory the council, masterminded by Count Arvid Horn, deliberately stirred up public discontent and raised the demand for peace as a means to increase its own power, as well as the relationship of the estates to both king and council, "concentrating on tendencies that could lead to the new constitutional order of 1719" after the death of Charles XII (p. 196). Carl Lennart Lundquist makes use of some new archival sources and critically re-examines the interpretations of several earlier historians. He organizes the study topically around Sweden's major international and internal political problems during that crucial year.

The result is a highly competent display of source criticism in the best Swedish tradition, but the study nonetheless fails to draw any real conclusions. The book ends abruptly, leaving the implication that available sources neither prove nor disprove any particular thesis. One gathers that

Horn and his allies on the council sought to justify their peace initiatives first by conniving with Britain to impose its mediation, then by adding Charles's sister, Ulrika Eleonora, to the council, and finally by summoning a *riksdag*. The council opposed efforts by the estates, once assembled, to play an independent role in matters of taxation, succession, and practical diplomacy. That Horn and his allies employed conspiratorial methods emerges clearly; that these served ulterior motives, beyond the conclusion of peace while securing the council against possible repercussions from either king or estates, does not. Lundquist seems to endorse a kind of modified conspiracy theory. The estates clearly reveal a growing assertiveness at the *riksdag* of 1713-14, which would clearly seem to foreshadow the "revolution" of 1719, but Lundquist limits himself to demonstrating the inadequacies of the documentary evidence.

Despite its English translation, as part of an international series on parliamentary institutions, and brief explanatory notes at the back, the book remains a Swedish monograph for Swedish specialists; it presupposes a background not altogether accessible to scholars unable to read Swedish. As it amounts basically to *Quellenkritik für Quellenkritik*, its usefulness for comparative studies is limited. One misses the wider perspective that could have placed this episode into its European context. Although the translation is generally adequate, overly literal renderings of Swedish words and syntax are, at times, misleading.

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ERKKI MARKKANEN. *Maaseutuväestön varallisuusolot ja luottosuhteet Sisä-Suomessa elinkeinoelämän murroskaudella v. 1850-1914* [The Conditions of Wealth and Credit Relationships of the Rural Population in Central Finland during a Period of Changes in the Economic Situation in the Years 1850-1914]. (Studia Historica Jyväskylän, number 14.) Jyväskylä: Jyväskylän Yliopisto. 1977. Pp. 263.

The English summary of this book describes its theme as "the effects of industrialisation on the living conditions of the rural population in Finland during the first phase of industrialisation at the end of the 19th and the beginning of the 20th century" (p. 207). Erkki Markkanen's own statement in Finnish of his purpose is similar (p. 12), and his preface promises "clarification of the impact of industrialisation" (p. 5). This promise is not kept.

Even by 1910-11, the latest years for which data are given, central Finland was still rural; less than

two percent of those studied were industrial workers. The change to a money economy had barely taken place. The first banks came to central Finland only in the 1890s, and by 1910 they had not yet replaced private individuals as the major source of loans. At the end of this period commercial capitalism may have arrived in central Finland but hardly industrial capitalism. Industrialization elsewhere, including abroad, affected central Finland only by increasing the demand for local wood, but this is hardly what social scientists generally mean by "the impact of industrialization." Indeed, Markkanen acknowledges that he has intentionally confined his study to rural residents (p. 13). This study is probably significant for understanding Finnish society but, since eighty-five percent of the total Finnish population was still rural in 1910, its value for comparative studies of industrialization is, to put it mildly, minimal. The great Weberian theme implied by the preface never appears.

Markkanen has in fact written a reliable, if rather desiccated, answer to the important, but exclusively Finnish, question of what changes, if any, took place in the distribution of wealth in rural central Finland from 1850–1911. His data are hardly drawn from actual "living conditions," since they come from comparing parish death registers with legally required inventories of the assets and liabilities of those recently deceased. Computers seem to have processed these data. Markkanen finds that such inventories were not in fact filed after most deaths. This finding raises serious doubts about the general reliability of the Lutheran state church's clergymen in their bureaucratic capacity as the chief recorders of Finnish demographic data. Previously scholars have doubted only the pastoral devotion of the Finnish clergy. Now it seems that they neglected their function of scribe also. Finnish social history may have to be rewritten because of Markkanen's incidental finding. Bless his computers.

The central substantive finding of this book, that the economic gap between landed and landless farmers widened significantly in the decades before the Great War, is clear, persuasive, and important. Unfortunately Markkanen stops here, only vaguely hinting in one-half of one sentence that standards of living can affect social stability (p. 202). This is a rather oblique way of referring to the economic causes of the Finnish Civil War of 1918. Markkanen makes no connections between economic and political history, let alone between economic history and political science; but perhaps the bare bones of his statistical tables, models of their kind, are the most persuasive evidence this reviewer has ever seen of the inevitability of the Finnish Civil War. Nevertheless, writing about the

Finnish rural poor before 1918 without even mentioning Väinö Linna's great novel, *Täällä Pohjantähden alla*, is like writing about the urban poor in nineteenth-century England without mentioning Charles Dickens.

MARVIN RINTALA
Boston College

PAULA TUOMIKOSKI-LESKELÄ. *Taide ja politiikka: Kansanedustuslaitoksen suhtautuminen taiteen edistämiseen Suomessa* [Art and Politics: The Attitude of the Finnish Diet toward the Promotion of Art in Finland (1863–1974)]. (Historiallisia Tutkimuksia, number 103.) Summary in English. Helsinki: Suomen Historiallinen Seura. 1977. Pp. 328.

"Finland cannot gain anything by force, her survival depends on her cultural strength" (p. 317). This prescient observation of J. V. Snellman, a leading nineteenth-century statesman, might well serve as the motto of this study of the Finnish diet's role in promoting the arts. It is truly an ambitious undertaking: 328 pages set in seven-point type (one hopes this will not become the standard practice of the Finnish Historical Society), an average of some five footnotes per page, and twenty-nine pages of bibliographical data. Regrettably, its encyclopedic scope and style of writing make for difficult reading.

The artistic or cultural sector treated in the book is comprehensive: literature (original and translated), painting and sculpture, music, theater, architecture, films, photography and the graphic arts, education, travel and study grants, stipends, prizes, and support of numerous associations for the advancement of related endeavors. Financing came from two major sources, public funds and private foundations.

State support officially began in 1864, although the present study wisely goes back, at least in part, to the time when Finland became a Russian grand duchy in 1809. The book divides the long sweep of historical development into the following periods: the era of the autonomous grand duchy with its antiquated four-estates system (1809–1906) and its modern unicameral diet and progressively more disciplined political parties (1906–17); and the era of independence, divided into three chronological stages (1917–44, 1945–61, 1962–76). Paula Tuomikoski-Leskelä describes quite adequately the features of these successive periods, and she reveals impressive familiarity with recent research and contemporary historical methodology. The focus of her book, of course, is on parliament, particularly its Cultural Affairs Committee. The detailed analysis covers such relevant matters as the structure and procedures of the committee; the occupa-

tional backgrounds, geographical origins, sex, age, attitudes, and motives of the committee's members; and its activity in proposing culturally oriented legislation. Attention is also given to the cultural content of the various political party programs.

From the overwhelming details of this volume, one might single out a few noteworthy conclusions. During the estates period, a total of twenty-five art bills were proposed, of which (surprisingly enough) eight originated in the peasants' estate. During the period 1891-1900, theater received 51.7 percent of the appropriated art funds, music 26.9 percent, painting 16.3 percent, and literature 5.1 percent. From 1917 to 1974 an average of forty-nine bills were introduced in each parliamentary session for the arts, or about 6 percent of all legislation introduced. Of these, 21 percent were for theater and music respectively. During the unicameral period (1906-17), 54 percent of the art bills were introduced by three prominent cultural figures, E. G. Palmén, E. N. Setälä, and E.-S. Yrjö-Koskinen. Other members of the Finnish Literature Society, who made up 24 percent of the diet, were also influential. This situation was challenged by the rising Socialists, who argued that the society was "conservative and did not in the least advance the cause of sociological literature" (p. 121). In the postwar years the leftist political parties have dominated parliamentary cultural activity: they sponsored 61 percent of the bills and the Cultural Affairs Committee's thirty chairmen from 1945 to 1975 all came from their ranks.

There is an English-language summary, which unfortunately is insufficiently clear and suffers from grammatical and typographical errors.

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WERNER O. PACKULL. *Mysticism and the Early South German-Austrian Anabaptist Movement, 1525-1531*. (Studies in Anabaptist and Mennonite History, number 19.) Scottdale, Pa.: Herald Press. 1977. Pp. 252. \$12.95.

During the last few years it has become increasingly clear that sixteenth-century Anabaptism was complex both in its nature and origins. Werner O. Packull's scholarly study, a revision of his doctoral dissertation (Queen's University, 1974), contributes substantially to our understanding of these intricacies in the development of South German and Austrian Anabaptism in the 1520s and early 1530s.

In the first of two major theses, Packull maintains that early South German Anabaptism inherited much of its theology and apocalypticism from late medieval mysticism through Thomas Münt-

zer, Hans Denck, and Hans Hut; Packull singles out the concept of the immanence of God in man because its implications support Anabaptist disagreements with the theological assumptions of main-line Protestantism. Although Packull acknowledges the presence of Swiss Anabaptist influence in the area from the beginning, he holds that it was not decisive.

Packull sees Müntzer as the transmitter of cross mysticism and apocalyptic expectations, and Denck as the bearer of the tradition of the *Imitation of Christ* and the ethics of the Sermon on the Mount. Although Denck was affected by humanism and minimized the apocalyptic element, Packull believes that "Denck's spirit comes closer to Müntzer than to the Reformers and a significant direct influence by Müntzer on Denck is not at all improbable" (p. 178). The major link was Hut, whose conversion to Anabaptism did not mark a sharp break with the Müntzerian apocalypticism and its message of vengeance.

After Hut's death and the failure of his predictions, early South German-Austrian Anabaptism disintegrated under a profusion of leaders. Many of Hut's followers, disillusioned and persecuted, recanted. The remnant, according to Packull, went in three different directions. A portion escaped into the fantasies of the Dreamers and of Augustin Bader. A second group turned toward an evangelical sectarianism, a direction anticipated by such leaders as Jacob Dachser, Leonhart Schiemer, and Hans Schlaffer; it included both the Hutterites and the less rigid Marpeck circle, although neither represents a simple continuation of the early South German-Austrian movement. Those more nonconformist and ecumenically oriented, represented by Hans Bündlerlin and Christian Entfelder, moved into an individualistic spiritualism.

Packull's second thesis is that early South German-Austrian Anabaptism represented a transitional movement from medieval mysticism to protestantized sectarianism or spiritualism and from a revolutionary movement inspired by social grievances and frustrated by the defeat of the peasants to a more religiously centered movement.

Packull's scholarship is meticulous. His copious documentation reflects careful use of both published and unpublished primary and secondary material, with which he supports not only his own interpretations but also his objections to other points of view. His arguments for both theses are convincing.

Students of the radical Reformation will welcome this excellent study, which throws more light on the religious ferment out of which Anabaptism developed.

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WALTER GROSSMANN. *Johann Christian Edelmann: From Orthodoxy to Enlightenment*. (Religion and Society, number 3.) The Hague: Mouton. 1976. Pp. ix, 209. DM 43.

This is the first full-length study of one of the most interesting but least known figures of the early German Enlightenment, Johann Christian Edelmann, whose life and intellectual development from Lutheran orthodoxy to radical religious free-thinking the author carefully and critically reconstructs in relation to the complex political, social, and spiritual environment in which he lived. Walter Grossmann makes no secret of his sympathy for this "great iconoclast" (p. viii) who, born into poverty and never able to achieve financial independence, nevertheless familiarized himself with and ceaselessly questioned every important religious teaching of his day. Edelmann dared to publicize his enthusiastic endorsement of the dangerous views of Gottfried Arnold and Spinoza, which eventually caused his own downfall. Grossmann correctly estimates that Edelmann's best and best-known writing, *Moses mit aufgedeckten Angesichte* (1740), was the first book in German "which denies from beginning to end the entire 'Bible-faith' and with it the Christian dogmas—a book that openly confesses its adherence to the Spinozistic teachings of God and the world, and that, without any inhibition, cuts the ground from under the conventional ideas of wonder, providence, and prayer" (p. 111). And, in general, Grossmann does not exaggerate his claim that Edelmann's views were at the very center of the religious and philosophical controversies of the age.

Grossmann then raises the obvious question: why did such famous thinkers of the later *Aufklärung* as Reimarus, Lessing, and others, who shared and elaborated Edelmann's views, fail to acknowledge his pioneering work and their indebtedness to it? Grossmann suggests 1.) that they were sometimes careless in their documentation; 2.) that Edelmann was not their intellectual or literary peer; and 3.) that, in contrast to them, Edelmann deliberately addressed himself to a lower-class audience, mindful that religion should not be for the educated alone. But the most important and plausible reason that Grossmann advances is that Edelmann was too outspoken an advocate of Spinoza, which earned him the stigma of materialist and atheist and led to the official condemnation and public burning of his books in Frankfurt in 1750. He was allowed to spend his last years in Berlin only on condition that he cease publishing.

Actually, Edelmann always remained ambivalent on the question of materialism and went no further than to proclaim the oneness of creation, the humanity of Jesus, and "the divine nature of

reason" in a work bearing that title published in 1741. But Grossmann reminds us that as late as 1780 even the great Lessing risked his reputation and incurred the wrath of Moses Mendelssohn by declaring himself a Spinozist. "The fact is that in 1780 it was still hazardous to write or to say what Edelmann had dared to say in 1740" (p. 177). It seems, then, that some of the boldest of the later *Aufklärer* were quite ready to share Edelmann's most radical religious views, but that they were also understandably reluctant to share his fate.

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HERMANN KELLENBENZ. *Deutsche Wirtschafts-geschichte*. Volume 1, *Von den Anfängen bis zum Ende des 18. Jahrhunderts*. (Beck'sche Sonderausgaben.) Munich: Verlag C. H. Beck. 1977. Pp. 412. DM 34.

Hermann Kellenbenz is one of the most prominent names in German economic history. His studies of commercial patterns, rural industry, and state finances have contributed substantially to our knowledge of the early modern period. A work from his pen, which covers the whole sweep of German economic history, especially one explicitly committed to overcoming the "ideology-laden" approach traditional to the field and directed at an audience of nonspecialists, must therefore be taken seriously. The book's strength—a rich fund of factual information—reflects in part Kellenbenz's own contribution to the field. The book's weakness—a virtual absence of interpretation and analysis—reflects the continuing influence of traditional presuppositions.

When Kellenbenz ventures an opinion on a broad interpretive issue, he tends to bury it under a mass of exceptions, qualifying it very nearly out of existence. Thus at the beginning of the sixteenth century the discovery of a sea route to India, the rise of the Turks, and the discovery of America presented "Germany" with "new economic perspectives," whose impact, however, varied from place to place to such an extent that Kellenbenz finds it impossible to determine the overall direction of development (pp. 213, 291–95). The Thirty Years' War constituted a "great divide" in establishing a dualism of empire and territorial states, increasing divisions and placing limits on "internal" economic relations, but did so by emphasizing developments that "had emerged in the course of generations," and again Kellenbenz finds the effects of the war and reconstruction too varied to permit generalizations (pp. 296–97, 381).

Kellenbenz's explanations of specific institutional developments tend to be advanced on an *ad hoc* basis without theoretical or comparative justification. Thus the Church's campaign against usury

in the eleventh and twelfth centuries failed because "practical experience showed that nothing was attained by the prohibitions" (p. 93). Artisan guilds arose in the twelfth and thirteenth centuries because "urban artisan manufacture grew so strong in this period that it organized itself cooperatively after the pattern of the merchant guilds" (p. 111). In the early seventeenth century Hamburg "understood" the need to adapt to changing conditions and succeeded, while Lübeck did not "understand" and declined (pp. 268-69).

In general, however, Kellenbenz avoids explanation. Nowhere does he explicitly state whether political events decisively influenced economic development or vice versa, nor does he attempt to evaluate the impact of economic development on social structure. He offers no interpretation of the rise of cities, concentrating instead on their dates of foundation and legal status. Similarly, new technologies are not explained, but merely "emerged" or were "employed." Population and price changes are described but receive no demographic or economic analysis.

Kellenbenz describes seventeenth- and eighteenth-century German society in terms strikingly similar to his description of social relations in the twelfth and thirteenth centuries (pp. 93-98, 315-20), but the absence of an explicit analytical framework makes it impossible to determine whether or not he considers the differences significant. For a nonspecialist audience, at least an introduction to the various historical interpretations of these and other topics seems a reasonable request. To interest a specialist audience, a general treatment such as this should come to grips with the theoretical literature in each subfield.

Very few factual statements can stand independent of an interpretive framework, and attempts to do so often conceal implicit presuppositions. Kellenbenz continues to share the traditional German preoccupation with unity and the activities of central governments. He emphasizes and favors the imperial institution of the Middle Ages (chaps. 3-5), referring to the "national" monarchy of the tenth century and blaming Gregory VII for the investiture struggle (pp. 93-94). He regrets the loss of unity resulting from the rise of territorial principalities, and he appears to be looking forward to a new German unity in his one-sidedly favorable treatment of eighteenth-century Prussian mercantilism (pp. 312-13, 323, 332).

It is not an organizational quibble to say that this book would be more effective if organized around regions rather than around sectors of the "German" economy. Kellenbenz is forced by his unstated assumptions to treat regional differences as exceptions or deviations from an increasingly

elusive norm, rather than as what they are—the basic substance of German history.

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HORST DIPPEL. *Germany and the American Revolution, 1770-1800: A Sociohistorical Investigation of Late Eighteenth-Century Thinking*. Translated by BERNHARD A. UHLENDORF. Foreword by R. R. PALMER. Chapel Hill: University of North Carolina Press; for Institute of Early American History and Culture, Williamsburg, Va. 1977. Pp. xxiv, 448. \$19.95.

According to the foreword by R. R. Palmer, "this is surely one of the most important books ever written on the impact in Europe of the American Revolution." Europeans, of course, watched the American Revolution with great interest. After reading Horst Dippel's book, there should be no doubt that Germans especially shared this curiosity. There have been earlier attempts to deal with the subject of Germany and the American Revolution focusing on belles-lettres, journalism, and limited manuscript sources. But Dippel's book far exceeds its predecessors in scope and depth. He found the published materials (nearly one thousand titles) so extensive that he published a separate bibliography, *Americana Germanica 1770-1800* (1976). Vast research in public and private manuscript collections on both sides of the Atlantic has revealed a wealth of letters, diaries, and diplomatic dispatches, much of which was previously unknown.

In Germany, as in France, there was a great deal of enthusiasm and sympathy for the American Revolution, but it was generally uncritical, idealized, and based on little factual knowledge. Prior to 1776, ignorance prevailed regarding the climate, geography, and population of America, and there seemed to be little demand for information. But the outbreak of war, accompanied by the recruiting of troops in various parts of Germany, caused a marked increase in interest that was met by an outburst of publications on America. Although some intellectuals remained supporters of England, many of the reading public came to understand the events in the New World as a novel and revolutionary struggle for the universally valid ideals of liberty and political equality.

But in contrast to France, few Germans considered the American example applicable to their situation. Most members of the German middle class considered America remote and did not think of themselves as political activists. Only a few like Georg Forster and Peter Ochs were ready to "take up the challenge emanating from America" (p. 197). After the Revolutionary War, there was little

discussion of internal American affairs; the Articles of Confederation were barely known. Contrary to common belief, the French Revolution was not responsible for the low level of knowledge in Germany about the American Constitution of 1787. Instead, the events in France stimulated interest in questions about the formation of governments, which prompted interest in the American Constitution. Idealization of the American Revolution increased during the course of the revolution in France. America was seen as a bourgeois utopia. The typical American was educated, industrious, tolerant, benevolent, and virtuous; the most eminent Americans were Benjamin Franklin and George Washington.

The "American Dream," to use Durand Echeverría's term, began to disintegrate around 1798, precipitated by the dispute over the Alien and Sedition Acts. The Enlightenment paved the way toward a more sophisticated perception of America by increasing knowledge of the world. The new republic came to be judged more on its achievements and less on the aura of its revolution. An important result of German interest in the American Revolution was the arousal of political consciousness in the middle class.

Dippel concentrates primarily on the ideas of the German "bourgeoisie," that is, the middle classes who made up the large part of the reading public, and the author's strength lies in the description and analysis of these ideas. But his attempts at a wider sociological study, categorizing views on the American Revolution by region, age, and social position, are less convincing, although these few pages should serve to stimulate further research. Too little space is allotted to what could well warrant a separate work. Dippel might have included a comparison of the German reaction to the American Revolution with that of other areas in Europe, especially France. The section "Reform or Revolution in Germany?" would have been most appropriate for such discussion. Bernhard A. Uhlendorf renders an excellent idiomatic translation from the German that is almost free of awkward syntax and ambiguity, making the book highly readable.

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GÜNTER WOLLSTEIN. *Das "Grossdeutschland" der Paulskirche: Nationale Ziele in der bürgerlichen Revolution, 1848/49*. Düsseldorf: Droste Verlag. 1977. Pp. 386. DM 48.

At the time of the centennial commemoration of the Revolutions of 1848, nearly everyone was urging the recently defeated Germans to revive their liberal traditions, which were best exemplified

in the Frankfurt *Paulskirche*. But Lewis Namier objected that the example was embarrassingly unsuitable in at least one respect: the liberal Frankfurt parliamentarians had been extreme nationalists who had tried to create a *Grossdeutschland* of extravagant dimensions, one that would have engulfed many non-German nationalities that were themselves struggling for independence. If Adolf Hitler had only examined the record, said Namier with characteristic exaggeration, he "might well have found a great deal to extol in the *deutsche Männer und Freunde* of the Frankfurt Assembly" (1848: *The Revolution of the Intellectuals*, p. 152).

Günter Wollstein's book is an extensive and depressingly detailed confirmation of this thesis. He shows that the situation before 1848 had encouraged the unrealistically optimistic view that different national groups had no conflicts of interest, only a common enemy in the Metternich system. The nationalist uprisings of 1848 brought forth the "springtime of the peoples," which was scheduled to develop into a sunny summer of national fraternization. But Wollstein shows how swiftly the weather changed when German national ambitions crossed those of their neighbors. In Dutch Limburg, in Danish Schleswig, in Polish Posen, in Bohemia, Trieste, and the Tirol, the theme varied only slightly: ethnic, historical, and practical criteria for annexation were intermixed with crude appeals to German "national egotism."

The new Germany would be a mighty Central European power that would exercise hegemony far and wide. Its navy, to be built as soon as possible, would demonstrate German power to the world at large. Its close association with Austria, whether of a *kleindeutsch* or a *grossdeutsch* complexion, would facilitate German dominion over the Danubian lands ("our Texas, our Mexico," in the words of one deputy, reminding us of the baleful influence of our own exuberant expansionism). Then Germans would no longer have to emigrate abroad; they could colonize southeastern Europe under their own flag. The lesser *Nationalitätchen* of the region would be civilized by the experience. Though moderate or realistic voices were heard now and then, and though there were important differences among the various political groups, this provocative and chauvinistic attitude affected conservatives, liberals, and democrats alike; no corner of the chamber was entirely immune. But Frankfurt's flag-waving generated apprehension throughout Europe, according to Wollstein; and this apprehension helped materially to revive public support for the old regimes and thus to destroy the prospects of the entire revolutionary movement of 1848.

The picture of an unrealistically expansionist Frankfurt Parliament is not new, but it has never

before been painted so thoroughly, systematically, and convincingly. Wollstein's purpose is to make possible a measured judgment of the revolution (p. 15). In particular, the book contributes to a revision of the widespread notion that the failure of the German Revolution of 1848 was an unmitigated disaster for the course of German history. It compels us to ponder the problems that might have been created if it had actually succeeded.

DONALD J. MATTHEISEN
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HEINZ HÜRTE, compiler. *Zwischen Revolution und Kapp-Putsch: Militär und Innenpolitik, 1918–1920*. (Quellen zur Geschichte des Parlamentarismus und der Politischen Parteien. Series 2, Militär und Politik, number 2.) Düsseldorf: Droste Verlag. 1977. Pp. lxxvii, 378. DM 118.

GERHARD W. RAKENIUS. *Wilhelm Groener als erster Generalquartiermeister: Die Politik der Obersten Heeresleitung, 1918/19*. (Wehrwissenschaftliche Forschungen. Militärgeschichtliche Studien, number 23.) Boppard am Rhein: Harald Boldt Verlag. 1977. Pp. vii, 270.

These two books deal with a period of German history that has been the subject of an unusually large number of studies. Under the circumstances it is perhaps not surprising that neither one contains major new revelations about those critical post-World War I months with which they deal.

Gerhard W. Rakenius's monograph corrects Wilhelm Groener's memoirs by showing that the collaboration of the army high command with the new government set up after the overthrow of William II was meant merely to safeguard the survival of the German state and to assure the orderly withdrawal of the army from France and Belgium. The agreement was not intended to secure political influence for the army from the hour of birth of the new republic, as Groener later claimed. Yet, on Rakenius's own showing, military efforts to influence politics set in almost immediately, so that this correction loses much of its significance.

Rakenius is little more convincing when he maintains that, at first, the high command was subordinate to the new government. And, although he finds the reliance of the government on the old army justified on the grounds that there was no time to create a new non-Communist military force (as Ulrich Kluge, *Soldatenräte und Revolution* [1975] has argued), he also makes clear, without saying so, that the government's dilatory response to civil-military issues played into the hands of Groener and his associates.

Heinz Hürten's documentation on the army's role in domestic politics between the armistice of

November 1918 and the outbreak of the Kapp Putsch in March 1920 is almost equally unrewarding. Nearly one-third of the documents deal with technical aspects of various states of siege. Others concern relations with civil authorities; but, since reactions from the latter with few exceptions are not reprinted, one gets a rather incomplete picture of this central issue. Similarly, the reports of the army commands on military and political conditions are mostly general summaries that tell a familiar story. The few lower-level reports that have been included are invariably more specific and, hence, more informative.

Some matters of interest do, however, emerge. Whatever the reservations of the officers toward the new republic—and they were, of course, strong and widespread—there is an explicit dissociation from all counterrevolutionary plans on the Right, which may help to explain the disarray of the Kapp Putsch from its very beginning. (There is nowhere any hint of the impending putsch in these documents.)

The overriding concern is always the radical Left. Yet opposition to leftist radicalism might have turned out to be costly. One general reported that most of his officers planned to resign if the Independent Socialists or the Communists should take over the government. He wanted to know "whether there are any guarantees that these officers will get honorable discharges and regular pensions. Who vouches for these guarantees?" General von Seeckt's reply was reassuring: while there were no such guarantees, there was also no likelihood that a left-radical government could come to power constitutionally. Against an unconstitutional government, on the other hand, the army would be free to intervene.

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GERALD D. FELDMAN and HEIDRUN HOMBURG. *Industrie und Inflation: Studien und Dokumenten zur Politik der deutschen Unternehmer, 1916–1923*. (Historische Perspektiven, number 5.) Hamburg: Hoffmann and Campe Verlag. 1977. Pp. 421.

GERALD D. FELDMAN. *Iron and Steel in the German Inflation, 1916–1923*. Princeton: Princeton University Press. 1977. Pp. xix, 518. \$26.50.

It is generally agreed that big business played an essential role in the destiny of the Weimar Republic, yet there is surprisingly little research on the actual decision-making process of the large concerns. Economic historians have concentrated on the issues of economic policy, and political historians have been reluctant to discuss economic questions at all beyond the statement that they did

matter. Filling this gap is, in my view, the chief merit of Gerald D. Feldman's remarkable book on contemporary German history.

This is a detailed study of how the leaders of the German iron and steel industry managed their enterprises, dealt with their customers, and acted in their relations with government and labor in a period of war, revolution, and economic crisis. It is, as Feldman explains, the offspring of a larger project on industry, labor, and the state in the early Weimar Republic, a sequel to his *Army, Industry and Labor in Germany, 1914-1918*. Both the difficulty of traversing a largely uncharted area of historical research and the abundance of source material deflected him from the original goal and led to this more specialized study. Whatever may have been lost in scope—and interesting indeed it would have been to see Feldman steer his history of German society through revolution and counter-revolution—is gained in depth.

The leitmotiv of the story is, in Feldman's own words, the "dilemma of how the special interests of privately owned corporate enterprises were to be reconciled with those of a society in the throes of a continuous domestic and international crisis" (p. 378). This dilemma led the iron and steel industrialists to an all-round struggle: conflicts with a government that they wanted to be strong enough to protect their interests and timid enough to yield to their demands; conflicts with finishing industries, which they needed as customers and as allies in industrial strife but which they exploited with high prices and bad contract terms; and conflicts with labor, which was to be cooperative and productive while being overworked and underpaid.

There was a basic continuity from World War I to the early years of the Weimar Republic in these issues of heavy industry versus finishing industries, control versus decontrol, and capital versus labor. Exploiting ruthlessly its key role in the economy, the iron and steel industry successfully pursued its particular interests against those of the finishing industries, government, labor, and society at large. Finishing industries, through high prices, financed much of the reconstruction of heavy industry; government, too, contributed with generous subsidies, while controls inevitably proved ineffective; labor, pacified with concessions in 1918, suffered subsequent defeat, so that most of the revolutionary gains (including the eight-hour day) were lost by the end of 1923.

More than a contribution to the history of the Weimar Republic and to the history of the iron and steel industry, the book is a case study in political economy, with great relevance today. The problem of democratic intervention, "of finding modes of regulation that do not persistently grant the wishes of those being regulated and of ex-

ercising authority without becoming authoritarian" (p. 469), remains yet to be solved. At two minor points I do disagree with Feldman. He insists that inflation began in 1916, when government expenditure permanently outstripped income from loans and taxation (p. 3). This is the contemporary budgetary view, but it is not tenable on modern, post-Keynesian grounds. Deficit spending began in 1914, and government was always ahead with a six-month deficit when the floating debt was funded even before 1916. Thus, inflation began in 1914 and was well under way by 1916; within the first two years of the war, the German mark had lost about one-third of both its internal and external value. The second point is the credit expansion in early 1924 (p. 449). I think it is incorrect to suggest a basic continuity from Havenstein to Schacht. There was no doubt about the priority of exchange stability since November 1923; still there remained the practical question of how much deflation was needed to stabilize the exchange rate and how much credit was needed to avoid economic disaster.

For the German edition Feldman has pooled his resources with those of Heidrun Homburg, a young German historian. The book contains a condensed, highly readable version of Feldman's study and a collection of fifty previously unpublished documents, carefully selected and annotated by Homburg. This very useful collection of documents makes the German edition a valuable contribution in its own right.

There are years of research behind this book, and several institutions have contributed to make it possible. The result is well worth it: covering entirely new ground, rich in detail, and highly readable, Feldman's book is one of the best, perhaps the best, on the early history of the Weimar Republic.

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LAWRENCE BARON. *The Eclectic Anarchism of Erich Mühsam*. New York: Revisionist Press. 1976. Pp. xii, 217. \$49.95.

Erich Mühsam (1878-1934) exemplified the bohemian intellectual committing himself and his art to the cause of revolution. He was a prolific political journalist, poet, and dramatist. His eloquent expression of revolutionary vision had special force for the profoundly rebellious who wanted no more compromise with oppression, no more temporizing until a distant better day. Thus he had a significant radical role in Germany, as did his friend Gustav Landauer, from whose thought Mühsam adapted a much modified version.

After a succession of adventures in radical agitation, he played an especially prominent part on the extreme Left during the 1918–19 Munich revolution. Drawing from the example of the soviets and the antistatist elements of Leninism, Mühsam conceived a libertarian regime of federated workers' and soldiers' councils, and this was very briefly attempted in Munich. He thought such a regime could be realized through the agency of Bolshevism, as did his libertarian counterparts in Russia. Like them he was soon disillusioned with the Bolsheviks, but he continued to agitate on behalf of this revised anarchism until the Nazis came to power. Immediately after the Reichstag fire they arrested, tortured, and killed him.

Much can be seen in Mühsam's career that will illuminate German revolutionary history, and ideas rather like his conception of workers' councils have assumed substantial importance in recent years. Unfortunately, Lawrence Baron does not manage at all well in his effort to reveal the anarchist to us, except in a final chapter. There he does give a clear, integrated view of Mühsam's "Bolshevist anarchism." Baron is a journeyman familiar with his craft's techniques without having mastered them. In place of deftness he gives us a heavy hand and short sight.

This is a straightforward intellectual biography, presenting the particulars in chronological order. Although he offers connective links between ideas and something of their context, the integrating effort is not effective. The book is more a series of little bits, ordered in some way but without a fully deliberate, coherent form. Too much is omitted for a complete picture. Baron takes each particular idea and says only enough about it and its matrix to render it initially intelligible, but without its full realization as part of a whole. Moreover, the author has chosen not to say much of Mühsam as creative artist or in any role after 1919; such exclusion is scarcely justifiable if the result is so short and insufficient a book. We learn too little from it of what Mühsam thought, and much less still of his place in history.

Baron shows himself unable to achieve a constructive distance from Mühsam or independence from the secondary authorities he uses abundantly. He may criticize faults, but does not develop a stance of his own. He has been thorough enough in his research, primary and secondary, but he does not transform it into his own creation. And his prose is labored, his expressions too often trite or clumsy. Mühsam may be exceptionally difficult to handle effectively; Baron certainly was unable to do so.

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LEWIS D. WURGAFT. *The Activists: Kurt Hiller and the Politics of Action on the German Left, 1914–1933*. (Transactions of the American Philosophical Society, number 67, part 8.) Philadelphia: The Society. 1977. Pp. 114. \$12.00.

Weimar Germany's left-wing intellectuals have both fascinated and frustrated scholars during the last decade. Since the late sixties, a number of important studies have appeared on the subject (for example, works by Deák, Poor, Angress, Gay, Mosse, Jay, and Lunn). These studies have analyzed the heterogeneous group of poets, polemicists, and publishers whose trenchant critiques of society reflected the diversity of ideas during Germany's troubled years preceeding and following World War I.

Lewis D. Wurgaft's monograph is an attempt to describe the ideologically ambiguous ideas of another left-wing intellectual, Kurt Hiller. Hiller free-lanced for a variety of journals, ranging from the expressionist *Die Aktion* and *Pan (II)* during the prewar years, to Weimar's *Weltbühne*. He also edited a radical journal, *Das Ziel*, which could hardly flourish under wartime censorship and only published two volumes before government confiscation. In addition to his affiliation with expressionist and leftist journals, Hiller is perhaps best known for his attempt to organize intellectuals into the short-lived *Rat der Geistigen Arbeiter* during the 1918–19 revolutions.

Wurgaft's study is the first to analyze extensively the progression of Hiller's socialist, pacifist, and intellectually elitist ideas. In addition, it is the first examination of Hiller's ultimately fruitless attempts at political activism. Hiller consistently advocated an idealistic *Logokratie*, a program that can be traced back to Plato. His utopian dream of uniting *Geist* and political power was doomed to failure during the turbulent years following Germany's military defeat. The quixotic visions that committed intellectuals to such activism quickly disintegrated into impotent factional strife. In 1921, Kaethe Kollwitz evoked the subsequent disillusionment experienced by many on the Left following the revolutions: "I . . . would have been capable of acting in a revolutionary manner if the real revolution had had the aspect we expected. But since its reality was highly unideal, and full of earthy dross . . . we have had enough of it." Hiller's idealism and the "earthy dross" of Weimar politics proved unbridgeable.

The book contains several intriguing ideas, including a major one that Hiller's activism "tended to obscure the customary distinction between the intellectuals of the left and the right—a tendency shared by many of the radical movements which followed them" (p. 11). This position reaffirms the

philosophical ambivalence of the Left that others, such as Deak, Poor, and Mosse have also noted. But the important observations made in the study are lost in a welter of digressions. The first two chapters, in particular, make for tortured reading since the author introduces many ideas and individuals in apparently random order. Obviously numerous philosophical and esthetic strands fed into activism, and Wurgaft rightly wants to indicate these. But without smooth transitions the information on antecedents is often very confusing to the reader. In addition to the early chapters' questionable organization, the prose is wooden and lifeless. If any group of Germans wrote passionately and exuberantly, it surely was the expressionists. But the poetry of their political zeal lies buried under longwinded, convoluted sentences. Furthermore, Hiller's own biography is limited to a few scanty details. Since he was unorthodox in his politics, his sexual practices, and his esthetics, Wurgaft's account would have been more interesting had he linked Hiller to the larger environment of Berlin's intelligentsia.

Aside from these reservations, the book is a valuable contribution to the literature of Weimar Germany. And as we have seen in the recent student protest movements, Hiller's radicalism was not merely a unique manifestation during war and revolution; similar critiques, echoing activism, have resurfaced during subsequent decades of the twentieth century.

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JOAN CAMPBELL. *The German Werkbund: The Politics of Reform in the Applied Arts*. Princeton: Princeton University Press. 1978. Pp. xii, 350. \$20.00.

The first to plan a book on this important movement was one of its leaders—Theodor Heuss. But he became president of the Bonn republic, and we are well served by this excellently researched, well-written, and, considering the subject, beautifully produced book.

Werkbund, founded in 1907 by the social-liberal leader Friedrich Naumann, and the civil service architect Hermann Muthesius, tried to transplant the principles of John Ruskin and William Morris to Germany. Combating industrial *kitsch*, it tried to bring good taste and workmanship back to household implements, furniture, and housing, in order to restore an arts-and-craft aspect to industrial goods and dignity to industrial labor. The *Werkbund* also tried to give German export goods a reputation for excellence in functional design, to contain the inroads of cheap imitations, to strengthen the national consciousness, and, fi-

nally, to weld employers and workers into a proud, creative, and harmonious social order—a tall order that justifies Joan Campbell's subtitle.

Indeed, the association was beset by internal debates, with its industrial sponsors and conservative architects like Hans Poelzig and Peter Behrens on one side, and romantics and expressionists like Gropius on the other side. Moreover, to the right, advocates of craftsmen and folk art tried to push it in the direction of Nazi esthetics, while on the left the clamor arose for "social building." Unfortunately, avant-garde design usually is more expensive than workers can afford, and the masses failed to respond to the call for "functional design."

Nevertheless, the *Werkbund* was most influential in shaping cultural policies during the Weimar period. It spawned the *Bauhaus* and the Dalcroze school of dancing. It became the official sponsor of the New Objectivity style, *Neues Bauen* and *Neue Sachlichkeit*, which Campbell calls the style of the Weimar establishment, meaning the style sponsored by progressive industrialists and bankers. But "it failed to solve the basic problem posed by . . . the democratic republic—how to reconcile intellectual and artistic leadership with the demands of a mass-based culture" (p. 226). Whether this could, or indeed should, be done was a question hotly debated; artists like Poelzig and Bruno Taut objected strongly to collaboration with industry, to standardization, to dictation by business or political interests, and to the plan of a unified, "German" style. But they were not pleased to find themselves aligned with yahoo advocates of folk art, and Campbell makes a good case for the middle line of Theodor Heuss—to teach both industry and consumers good taste in industrial products and to work along with the powers-that-be for the promotion of good causes.

Unfortunately, this led him and his friend Ernst Jäckh (the chairman of the *Werkbund*) to believe that they could also collaborate with the Nazis. They were quickly disabused of such illusions. In 1934, the *Werkbund* was absorbed into the Nazi *Kampfbund für Deutsche Kultur* (Fighting League for German Culture), and its slogan, "Joy in Work" was appropriated by the Labor Front's "Beauty in Work" and "Strength Through Joy" divisions. It is true that the folk art and handicraft advocates were also betrayed by Hitler, who embraced a neoclassical and functional style. The book affords us a close insight into the intricate relations of art with politics in an industrial society.

HENRY PACHTER
Rutgers University

KLAUS SCHOLDER. *Die Kirchen und das Dritte Reich*. Volume 1, *Vorgeschichte und Zeit der Illusionen*, 1918–

1934. Frankfurt am Main: Propyläen. 1977. Pp. ix, 897. DM 48.

With the publication of this first volume by a professor of modern church history at the evangelical theological faculty of the University of Tübingen, the historiography of the German Church struggle has come of age. It began in the late 1930s when writers outside Germany produced accounts of religious persecution in Hitler's Third Reich. Following 1945 the survivors of the Church struggle began to publish their own accounts, based in part on memories still fresh and in part on such documentation as was available amidst the chaos of a ruined country. Under these circumstances it was hardly surprising that this literature failed to satisfy the standards of scholarly objectivity.

Equally understandable, but less excusable, were the distortions of the often savage revisionist literature that followed. By the 1960s this had persuaded many people (including not a few professional historians whose critical faculties in this instance abandoned them) that the German Churches had been tools of fascism, and that Pope Pius XII was coresponsible for the death of six million Jews. Mostly overlooked in the strife of tongues was a characteristic shared by the revisionists and their opponents: a majority on both sides had produced works of advocacy rather than objective history.

The revisionists' principal contribution was to compel the publication of a mass of documentation for which we might otherwise have had to wait for decades. Church sources thus revealed often sound like collaboration—at least to those unversed in the codework dialectic fostered by all totalitarian dictatorships. Nazi sources, on the other hand, register almost unremitting Church resistance. Of the many attempts, especially in the last decade, to make sense of this contradictory evidence, Klaus Scholder's is the most ambitious and, though flawed, the most successful.

He is the first German historian to study the Catholic and Protestant records together. His Catholic chapters are based almost entirely on published documents and are far briefer than the account of Protestant developments, to which fully two-thirds of this somewhat over-long and occasionally repetitious volume are devoted. Scholder justifies this disproportion by the greater internal unity of German Catholics, who experienced nothing approaching the confusion and impassioned controversy that convulsed German Protestants as they struggled to find the appropriate response to Hitler. This Protestant story is told largely from unpublished sources, not a few of which Scholder discloses for the first time. The result is a narrative

of, at best, gripping interest, which succeeds in making sense of an immensely complicated story in a way not achieved by any previous work known to this reviewer.

Though Scholder's decision to treat both major confessions together is commendable, his Catholic chapters are often weak. Of this two examples must suffice. In his account of events in early 1933 Scholder follows a chapter entitled "Die Kapitulation des Katholizismus" with another on "Die Selbstbehauptung der evangelischen Kirche." The evidence he marshals fails, however, to support this implied contrast; for nowhere does he report on the Catholic side anything remotely approaching the ecstatic enthusiasm for Hitler which, as Scholder amply documents, for a time gripped large sections of German Protestantism.

Less satisfying still is his lengthy account of the Concordat of July 1933 between the German Reich and the Holy See. Repeatedly Scholder insists that this agreement resulted from an undocumented secret deal made in early March between Hitler and Monsignor Kaas, then head of the Catholic Center Party (*Zentrum*). This allegedly called for the *Zentrum* to supply the crucial votes for the Enabling Act, in return for Hitler's promise of a concordat which would doom the party to extinction. Among much evidence contradicting this hypothesis is a well-documented fact mentioned by Scholder only in a footnote. Throughout the concordat negotiations Kaas fought tenaciously for his party by refusing to accept the exclusion of the Catholic clergy from party politics (for the sake of which alone, on Scholder's own account, Hitler desired the concordat). When, in June 1933, the democratic parties collapsed, what had previously been an unacceptable Church concession became a protection for the clergy against absorption by the only party remaining—Hitler's.

Scholder deserves unqualified recognition for being the first German historian really to face the Jewish question. While documenting with severity and scholarly rigor the Churches' failure in this area, he succeeds in making it, for the first time, understandable in its historical context. The churches' silence arose less from cowardice than from preoccupation with their own desperate struggle for survival amid a reign of terror for which nothing in their previous experience had prepared them.

The failure was one of perception, but it did not go unprotected at the time. Scholder cites a Catholic priest who, in April 1933, pleaded with Cardinal Faulhaber of Munich for a bolder line, saying that "only the spirit of the confessors and martyrs" could restore Christian credibility. Why, Scholder asks, did this clear perception, which was articulated by many Protestants as well, fail to convince

Church leaders at the time? His answer: "Difficult though it is for later generations to comprehend, such truths have never seemed obvious to all Christians. This fact should give pause to those today who stand in judgment and condemnation" (p. 345).

We eagerly await the publication of his concluding, second volume.

JOHN JAY HUGHES
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RAIMUND BAUMGÄRTNER. *Weltanschauungskampf im Dritten Reich: Die Auseinandersetzung der Kirchen mit Alfred Rosenberg*. (Veröffentlichungen der Kommission für Zeitgeschichte. Series B, Forschungen, number 22.) Mainz: Matthias-Grünewald-Verlag. 1977. Pp. xxxii, 275. DM 68.

One can argue that National Socialism functioned as a surrogate religion for many Germans who rejected traditional Christian creeds as reactionary or sought a new faith to fill the void. The "Church father of National Socialism," as Hitler once called him, was Arthur Rosenberg (1893-1946), whose weltanschauung was presented in 1930 in *Der Mythos des 20. Jahrhunderts*, a long and rambling attack against the churches (especially the Roman Catholic) for being "spiritually enslaved" and thoroughly Judaized. In the place of Christian love and emphasis on compassion, humanity, and the equality of all before God, Rosenberg proposed a religion of honor (*Ehre*), with its center in race consciousness and *Charakterstolz*. He minced no words in his rejection of the two Church traditions: "The myth of the Roman deputy of God must be overcome, just like the myth of the 'holy scriptures' in Protestantism. A new binding, forming middle point lies in the myth of the *Volk* soul. To serve it is the duty of our race" (p. 67). Rosenberg's ideas were influential at the middle and lower levels of the party faithful, although the Nazi elite often ignored him or followed a more pragmatic policy of paying lip service to "positive Christianity."

Raimund Baumgärtner's book was submitted as a dissertation at the University of Munich. It relies on some unpublished sources, including Bundesarchiv material, various German state archival holdings, and correspondence and interviews. In addition, the author used published materials related to the debate mounted by the Church over the *Mythos* and some forty books, pamphlets, and collections of speeches by Rosenberg. The book is not intended to be a general biography of Rosenberg (Robert Cecil's 1972 work, *The Myth of the Master Race*, comes closer to this goal), and therefore the sources used were more than adequate for covering the religious debate.

This work is divided into two main sections. The first analyzes Rosenberg's intellectual and party development, the contents of the *Mythos* and other of his works, and his influence on the membership of the Nazi Party. The second deals with the reactions of the two branches of the Church in Germany: Roman Catholicism and Protestantism. Baumgärtner effectively demonstrates that the Catholic opposition to Rosenberg's neopaganism was more consistent and united (both before and after 1933) than was the Protestant. In the latter case, a sizable proportion of pastors in the so-called German Christian wing of the Church were either willing to give Rosenberg a hearing or were, in some cases, supporters of his ideas. Many Protestants were willing to countenance the decanonization of the Old Testament, the de-emphasis or elimination of St. Paul's influence in theology, the "cleansing" of Jewish terminology and ideas from the liturgy and hymns, and the creation of a "German" style of piety closed to non-Aryans.

The book, which is well written and is an important contribution to *Kirchenkampf* history, has a special significance in an additional sense. Because it was official party policy to distinguish between Nazi ideology and religion as such, the Rosenberg-Church confrontation (which lasted until 1939) provided an unusual opportunity for a public debate of the ideological bases of the regime.

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The School of the Ozarks

VOLKER LOSEMANN. *Nationalsozialismus und Antike: Studien zur Entwicklung des Faches Alte Geschichte, 1933-1945*. (Historische Perspektiven, number 7.) Hamburg: Hoffmann and Campe Verlag. 1977. Pp. 283. DM 48.

Volker Losemann has written a detailed study of the impact of National Socialism on ancient historians and classical philologists. He concludes that, although a very serious loss of talent and training resulted from the dismissal of Jewish scholars early in the regime, on the whole the profession maintained its scholarly standards and resisted the efforts of various Nazi agencies to give the study of ancient history a new ideological orientation or to interfere in personnel decisions. Yet Losemann's description of scholarly careers and academic politics during the Third Reich is scarcely edifying. Examples of careerism, opportunism, institutional and personal rivalry, empire-building, in-group solidarity against outsiders, and the deliberate cultivation of contacts with powerful political figures are abundantly documented, even though Losemann has carefully avoided easy moral judgments.

After a brief sketch of the rather vague but

markedly varying opinions concerning the value of the study of antiquity held by leading Nazis and a more substantial chapter on the effects of the purge of "undesirable" scholars from German universities, Losemann turns to his main concern: the institutional and personal factors that impeded the full nazification of the disciplines of ancient history and classical philology, even though much superficial accommodation took place. He shows that, despite efforts by Alfred Rosenberg or the leaders of the *Dozentenbund* to win favorable treatment for candidates chosen for ideological reasons, the profession, with some exceptions, succeeded in upholding respectable academic standards in making promotions and new appointments. In fact, officials in Rust's Ministry of Education normally confirmed the appointments recommended by specialists in the field forwarded through customary channels and thus blocked attempts of party organizations to gain control of personnel selection.

While the tenacity of the academic profession in rejecting interference from outsiders was a real factor in preventing complete nazification, professionalism did not preclude some conspicuous examples of *Selbstgleichschaltung* (for example, Franz Altheim and Richard Harder). Losemann's investigation makes it clear that a systematic reorientation of teaching and research failed primarily because of the intense personal and bureaucratic rivalries inside the Nazi establishment itself. Above all, Rosenberg and Himmler could not agree. Each competed in the creation of a stable of favored scholars by taking an interest in their careers, funding research, commissioning scholarly writings, and establishing special study groups and institutes. The protégés transmitted their patrons' rivalries into the academic world, occasionally amplifying them by personal vendettas of their own.

Losemann's overall conclusions conform to a pattern established by Helmut Heiber in his work on Walter Frank, Reinhard Bollmus on the *Amt Rosenberg*, and Michael Kater on the *Ahnenerbe*, although many of his details and some points of interpretation are new. Specialists particularly interested in how higher education adjusted to the Nazi regime will want to consult this monograph; other readers are likely to find its focus very narrow.

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DONALD M. MCKALE. *The Swastika Outside Germany*. Kent, Ohio: Kent State University Press. 1977. Pp. xvi, 288. \$14.00.

Unlike communism, which claims to address itself to international and universal economic and political

problems, German National Socialism was handicapped throughout its existence. In spite of some attraction to national socialist and fascist movements in other countries, it fundamentally lacked appeal outside of Germany for anyone but Germans, and not for many of them at that. If any proof of this truism were still needed, Donald M. McKale's well-researched book on Nazi organizations and fellow travelers outside Germany provides it. His work illuminates another aspect of the National Socialist regime in Germany and contributes one more piece to the mosaic that was the Third Reich.

It was inevitable that the Nazis would attempt to enlist German citizens living abroad in their cause. Disappointed by the Weimar Republic's failure to solve its problems, many Germans abroad rejoiced at what they perceived initially as a positive direction in German affairs. From about 1930 onward, the Nazis, mainly under the direction of Ernst Bohle, began to capitalize on this sentiment through establishment of party organizations and infiltration of ethnic and cultural German organizations. Following Hitler's maxim that "once a German, always a German," the Nazis made little distinction between German citizens living abroad and people of German descent who had assumed other citizenships. Thus counting thirty million people as potential followers, the Nazis made strenuous efforts to recruit them, especially in countries with large German minorities. However, the Nazis' blandishments fell on mostly deaf ears with ethnic Germans, and even with German nationals their success was pathetically meager. By 1937, only five percent of German citizens abroad had become party members.

In fact, the methods employed by the Nazis—blackmail, noisy demonstrations, displaying of swastika flags, and wearing of Nazi uniforms—and the sterility of their ideology won Germany few friends. German propaganda led governments, especially in Britain and the United States, to believe that Nazi organizations were much more powerful than they were. By 1939 most countries had become hostile to Germany and had banned Nazi organizations.

Bohle's lack of success with Germans abroad was mirrored in his failure to take over the German foreign service, whose conservative and traditional diplomats largely succeeded in fending off Nazi domination. This attitude did not change in 1938 when Ribbentrop became foreign minister, because he resented Bohle's efforts for personal reasons. Indeed, Bohle expended much of his energy on the constant internal warfare among party branches and leaders, that become a characteristic of Nazi Germany. Bohle went to trial at Nuremberg in 1947 and eventually received a sentence of

five years imprisonment for atrocities against civilian populations and for membership in the criminal SS. His short sentence indicated the minor role that he played in the larger arena of Nazi Germany.

A few minor points in translation or interpretation should be noted: *Volksgruppe* is not a racial group, and why is it that every campaign the Germans fought was "brutal" and why was Antonescu a puppet? Is there no other explanation for his or Romania's conduct? More important, why is there no mention of Fritz Kuhn after May 1939? What happened to him, and what was the eventual fate of the German American Bund?

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ALFRED M. DE ZAYAS. *Die Anglo-Amerikaner und die Vertreibung der Deutschen: Vorgeschichte, Verlauf, Folgen*. Introduction by ROBERT MURPHY. Munich: Verlag C. H. Beck. 1977. Pp. 300. DM 24.

One of the greatest tragedies that resulted from the debacle of the Second World War was the mass flight and expulsion of Germans from Central and Eastern Europe. This greatest of forced migrations in history involved over twelve million Germans, chiefly from Czechoslovakia and former German lands that were taken over by Poland at the end of the war. Over two million refugees perished during the flight.

The story of the postwar expulsion of Germans is not well known in this country. Although many German publications (some in English translation) have documented this mass movement that occurred as a result of Allied wartime decisions, it has received relatively little attention from American historians and even less from the mass media, partly because of a lingering feeling that Germans were merely receiving their just retribution for the horrors perpetrated by them on their neighbors. It is gratifying, therefore, that Alfred M. de Zayas, a young specialist in international law, who was deeply moved by the writings of Victor Gollancz on conditions in postwar Germany, undertook to trace the origins and the course of the expulsions and, in particular, to examine the share of Anglo-American coresponsibility for the policy that provided for the "transfer" of German populations. In addition to using the published German, British, and U.S. documents, memoirs, and secondary literature, the author has examined unpublished sources at the Bundesarchiv in Koblenz and Freiburg, the Institut für Zeitgeschichte in Munich, the archives of the League of Nations and the International Red Cross in Geneva, the Public Record Office and the Polish Institute and Si-

korski Museum in London, and numerous other European archives. He has also conducted interviews with survivors and former government officials who were involved in the policy decisions regarding the postwar removal of Germans back to the dismembered home country.

As is well known, the "total expulsion of Germans" (Winston Churchill's words) was the corollary of the Allied decision to assign the lands east of the Oder River (eventually the Oder and Western Neisse Rivers) to Poland in compensation for the loss of territories east of the so-called Curzon Line to Russia. The general outlines of these arrangements were laid down at Teheran and Yalta, with Churchill and FDR agreeing rather lightly to Russian and Polish demands, while the final details for a supposedly "orderly and humane" transfer of Germans from Poland, Czechoslovakia, and Hungary were decided at the Potsdam Conference. When Allied leaders discovered that the expulsions continued to be carried out in the most precipitate and ruthless manner, causing human misery of astronomical dimensions in the occupation zones, they attempted to moderate the process by protesting to the governments of Poland and Czechoslovakia but with little effect. Among those who expressed moral indignation over these excesses was Robert Murphy, the U.S. political adviser for Germany, who wrote a strong memorandum on the subject to the Department of State (quoted in this edition), and who also wrote a foreword to the book. The work concludes with chapters analyzing the impact of the *Ostpolitik* and the eastern treaties, as well as the Conference on Security and Cooperation in Europe, on the status of the Oder-Neisse border and the lands beyond.

De Zayas writes with sympathy for the refugees and moral indignation over what he, as an international lawyer, concludes was another "crime against humanity," but he strives to show how Allied decisions regarding postwar Germany were the product of many factors, such as horror over Nazi atrocities, the passions of war and victory, and considerable ignorance on the part of Anglo-American leaders regarding the actual state of affairs in Central and Eastern Europe.

This German edition, which followed the English edition (1977) by only a few months, is enriched by additional data and documentation. There is a comprehensive bibliography and a selection of pertinent illustrations.

CARL G. ANTHON

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IGOR KARAMAN, editor. *Zur Geschichte der Burgenländischen Kroaten: Vorträge an der wissenschaftlichen Konferenz, abgehalten in Zagreb am 25-26. März 1974*.

Zagreb: University of Zagreb, Institute of Croatian History. 1977. Pp. 93. ND 30.

During the last two decades Europe's ethnic minorities have increasingly become an object of public attention. There appears to have been a rise in the self-consciousness of persons belonging to such groups. At the same time specialists from a number of disciplines have begun to advocate positive steps to assure their protection and preservation. These trends affect sharply different types of minorities: the noncontiguous segments of fully mature, major nationalities, linguistic-cultural enclaves that have endured largely unnoticed since antiquity, and the dispersed fragments of historically active, if less prominent, peoples. The Croats of the Austrian federal province of Burgenland fall into the last category and, along with Carinthia's Slovenes, are of particular interest because of bitter disagreement over them between Belgrade and Vienna.

The modest volume under review represents the proceedings of a scientific conference held at the University of Zagreb's Institute for Croatian History. Paradoxically, the colloquium was officially sponsored by the Burgenland provincial and Croatian republican governments. The emphasis of the meeting, attended by Austrian German and Austrian Croatian, Yugoslav Croatian, Magyar, and Slovak scholars, was not upon the current political controversy but rather its historical setting, that is, the northern diaspora of the Croats that commenced in the sixteenth century. (The term "Burgenland Croat" also encompasses residual ethnic islands in Hungary and Slovakia). Each contribution has been published in the language in which it was presented—German, Serbo-Croat, or Slovak—along with a translation into either Serbo-Croat or German. Thus in every instance there is a German version of the material. The individual titles and authors are: Igor Karaman, "The Current Status of Research, Including Results to Date, on the History and Culture of the Burgenland Croats in the Institutes of the Croatian Socialist Republic"; August Ernst, "Scholarly Literature Regarding the History and Culture of the Burgenland Croats"; Felix Tobler, "Primary Sources Relating to the History of the Burgenland Croats in Austrian Archives, Libraries, and Museums"; Ivan Filipović, "Materials Concerning the Burgenland Croats Available in Zagreb"; Vera Zimányi, "Croatian Migrations of the Sixteenth and Seventeenth Centuries in Specialized Magyar Literature: Further Research Opportunities"; and Květa Kučerová, "Slovak Historiography on the Problems of Croatian Colonization." The quality of the papers is uniformly high, and the book is an

essential addition to any library collection on the subject.

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ESMONDE M. ROBERTSON. *Mussolini as Empire-Builder: Europe and Africa, 1932-36*. (The Making of the 20th Century.) New York: St. Martin's Press. 1977. Pp. 246. \$17.95.

Several generations of liberal antifascist historians, concentrating on the 1930s and 1940s, have argued that Mussolini was an arbitrary improviser who projected his domestic illusionist politics onto foreign affairs. There is no doubt that after 1936 Italy was reduced to isolated, then dependent, status. In the years before, however, Italy was enmeshed in European politics, and Mussolini acted among the powers. His statecraft is Esmonde M. Robertson's subject.

Since the search for Mussolini's motivations is often limited to evaluating circumstantial evidence, a reliable diplomatic history such as this is useful, for it establishes what Mussolini had to take into account internationally. Robertson does not discuss in much detail the considerable evidence available on the influence of ideology, bureaucracy, leadership, economic and military conditions, and, indeed, Mussolini's personality on foreign policy. The international perspective he establishes, however, shows us the relationship of Mussolini's decisions on foreign affairs to his plans and to changes in the international domain.

Faced with an increasingly incoherent configuration in the early 1930s, Mussolini realized he must give up his ambition to play a determining role in Europe—the policy of "equidistance" meant to place Italy as the swing power between the three major states. As changes took place in relations with Germany, Austria, and Yugoslavia, he discarded the hope that Italy could become protector and exploiter of the lands of the Habsburg heritage.

The turn away from the Alps and the Adriatic, the turn to Africa, was a major shift, although one with strong nationalist roots. It was not an illogical decision. Mussolini thought Europe would let him have Ethiopia, and so in fact he was given to understand, although by then he did not much care about European opinion. The Ethiopian war, however, gave rise to sanctions, the unexpectedly serious isolation of Italy, and the closure of alternatives in Europe. Hitler, looking back, said the Axis began in September 1935, the eve of the Ethiopian invasion, and Robertson (who ends his analysis at this point) suggests this is correct. I would

put the date later, but the general point remains that in 1936 Mussolini's interest in and Italy's capacity for an influential and independent policy in European affairs no longer existed.

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SANDRO ROGARI. *Santa Sede e fascismo: Dall'Aventino ai Patti Lateranensi*. Preface by GIOVANNI SPADOLINI. (Documenti per la Storia del Nostro Tempo, number 6.) Bologna: Arnaldo Forni Editore. 1977. Pp. 314. L. 10,500.

In little more than a decade the study of Italian Fascism has undergone something of a revolution: the opening in the early 1960s of the Italian state archives (which may claim one of the most enlightened access policies in Europe) has exposed Fascism to a degree of rigorous, scientific scrutiny that was once impossible. But because of the closed doors at the Vatican archives, what is true in general does not hold for the specific history of relations between Fascism and the Catholic Church. Yet, despite the problem of evidence, what Ernesto Rossi, Gaetano Salvemini, and others wrote about the subject decades ago has held up remarkably well in broad outline, and recent and more dispassionate Italian scholars (De Rosa, Margiotta Broglio, Spadolini, and Scoppola) have continued to fill in details and refine interpretations.

This ably crafted study by Sandro Rogari relies heavily on the Catholic press in lieu of the unavailable Vatican sources, but makes extensive use of Fascist police documents and other unpublished material from the Italian archives. Leaving aside the intricate and familiar details of the high-level negotiations that led to the Lateran Pacts, Rogari deals instead with the "collateral" history of *Azione Cattolica* and other confessional organizations in the crucial period 1925–29. The result is a carefully balanced reconstruction of the groping, tense, and uncertain search for a *modus vivendi* that dominated the encounter between the Catholic world and civil society in Italy during the birth of the Fascist regime.

The theme that permeates this book is the logic of survival: faced with the dangerous and violent struggle for power that consumed post-1919 Italy, and confronted with the growing totalitarian ambitions of the regime, the Vatican determined to withdraw completely from politics and concentrate instead on reinforcing its spiritual and moral hold on the Italian people. The determination to survive the consolidation of Fascism brought the calculated abandonment of the *Partito Popolare Italiano*, whose existence threatened to draw the Church into the vortex of political conflict, and the

concurrent decision to revitalize *Azione Cattolica* as an apolitical instrument of Catholic education and propaganda. Rogari's focus on *Azione Cattolica* reflects the Vatican's belief that successful dealings with the regime required a strong, well-organized Italian Catholic movement directly dependent on the papacy.

Vatican policy hinged on several important assumptions: that Fascist ideological imperatives and the regime's totalitarian instincts were a clear and immediate danger to the interests of the Church in Italy; that the Church could not surrender the basic principle of absolute papal authority in matters of faith and ecclesiastical administration; and that a "realistic" bilateral agreement with the Duce was both necessary and possible. Pius XI readily gave in to Mussolini's intransigence on the territorial question in return for Catholic influence in the education of Italian youth and the independence of Catholic lay organizations. As a mutually reluctant compromise, the Lateran Pacts provided Mussolini with an immediate victory in his efforts to win popular consensus for his regime, but only in exchange for the long-term continuance of Church authority in Italian life.

Rogari's work is noteworthy both for what it reveals about the nature of Catholic reaction to Fascism and for the controlled equilibrium of its approach. Although it is fashionable in some historiographical circles to condemn scholars who strive to be "objective" in dealing with Fascism, Rogari astutely avoids the pitfalls of ideological polemics. His concern is not to demonstrate the existence of a logical nexus of reactionary forces known as "clerico-fascism"—forces which unquestionably did exist among some Italian Catholics—but to isolate and define the role of the Vatican in the heterogeneous world of Italian Catholicism. Just as historians have come to recognize that Fascism was not a monolith, Rogari has shown that Italian Catholic opinion was not synonymous with Vatican policy. While the Holy See subordinated diverse Catholic elements to its *raison d'état*, the papal flock that ranged from Luigi Sturzo to Egilberto Martire encompassed a wide range of discordant views. The Vatican's skill in balancing, manipulating, isolating, and emasculating these diverse forces was no less remarkable than Mussolini's ability to orchestrate his disparate menage of Italian fascisms.

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SAMUEL H. BARNES. *Representation in Italy: Institutionalized Tradition and Electoral Choice*. Chicago: University of Chicago Press. 1977. Pp. xi, 187. \$16.00.

The use of mass surveys in electoral analysis has long characterized the study of American politics. In recent years, scholars have applied this mode of analysis to other countries. To do so properly requires dealing with issues of cross-national importance, understanding the relevant local conditions, and overcoming the various technical problems associated with such research. When successful, one has a body of information useful in the analysis of general theoretical questions as well as of a particular election. Samuel H. Barnes has been successful.

This study is based on a national sample of voters, communal councillors, and parliamentary deputies. Interviews of the voters took place just after the 1968 national elections; those of the political elite just before. Barnes reports that the mass sample is representative of the voting population (though it would have been better to cite the relevant census figures, so the reader could see for himself). The 1968 election is used to typify the electoral patterns that prevailed until very recently. Most important of these was the continuity in results of elections: parties generally received the same portion of the vote in election after election.

During this period, electoral stability coexisted with cabinet instability and social and economic flux. Barnes distinguishes between the two spheres of politics, arguing that electoral politics has not been directly related to policy-making and elite politics. His analysis of electoral behavior places it within its theoretical context by developing the concepts of mobilization, representation, and political cleavage. At the same time, it complements the theoretical excursion by examining the relevant survey responses. In Italy, Barnes argues, the link to electoral behavior occurs through persistent traditions tied to political parties—a Marxist-socialist one linked to the Communist and socialist parties and a Catholic tradition associated with the Christian Democrats. Most Italians may be found within one of the two. The attachments to the traditions produce the electoral stability. More generally, Barnes maintains that the parties and the traditions help determine voting behavior in Italy. The parties are the traditional vehicles of electoral competition, but the traditions exist apart from the parties.

The distinction between the two spheres of politics is most useful in Italy, and probably elsewhere as well. For that reason, a more detailed analysis of the ideas and histories of the traditions would have been appropriate. The exclusive focus on the 1968 election seems to preclude that. Similarly, the relative importance of political party and social factors merits a more elaborate examination. As it stands, it is still not possible to resolve whether the

parties have maintained or have responded to the importance of the traditions, the widespread use of the left-right distinction, or the policy differences found among the electorate.

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CHARLES JELAVICH and BARBARA JELAVICH. *The Establishment of the Balkan National States, 1804–1920*. (History of East Central Europe, number 8.) Seattle: University of Washington Press. 1977. Pp. xv, 358. \$18.95.

This book by Charles Jelavich and Barbara Jelavich is a closely woven narrative history of seven Balkan peoples—Albanians, Bulgarians, Croats, Greeks, Rumanians, Serbs, and Slovenes—from 1804 to the end of the First World War. This was an important revolutionary period for Balkan peoples, the dying phase of four, some Balkan peoples say five, centuries of Turkish rule. This was the age when national movements, spurred on by economic development and the expansionist rivalries of great powers, led to the formation of five new nation states by these peoples in the Balkans. (The two exceptions were the Croats and Slovenes, who by some definitions would not be counted as Balkan peoples; they joined the Kingdom of Serbs, Croats, and Slovenes in 1918 but did not achieve separate states or autonomy.) This was also the age when Russian westward expansion became an important factor in Balkan history.

Nationalism, national cultural identity, and liberation from foreign rule became the overriding purposes of Balkan peoples in this period. This book provides a factual framework for this theme, covering some aspect of it in each of the nineteen chapters. Fourteen of these deal chronologically with the Serbian, Greek, Bulgarian, and Albanian revolutions and the states they brought into existence. Two of these chapters describe the Turkish history essential for an understanding of the interplay between nationalism and misrule in the final stages of the decline of Ottoman power in Europe. They also explain the economic, social, and political backwardness of the region; this, added to the strategic importance of the Balkans—a fact not stressed in this book—explains why the new states were immediately natural prey for great power—especially Russian and Austrian—rivalry and exploitation. The last five chapters cover relations between these peoples and those of adjoining regions and the developments which led to the post-1918 settlement. One later chapter deals with cultural developments, and one with what the authors call “the Balkan nationalities of the Habsburg Empire.” This latter chapter covers a great deal of

material including the history of the Slovenes, Dalmatia, the Military Frontier, Transylvania with its Magyar-Rumanian dichotomy, relations between Slavs and Magyars, the problems of Bosnia-Herzegovina, the development of the South Slav movement with its impact on both Hungary and the Habsburg monarchy, as well as much else. With its wealth of detailed information weaving cultural, social, and economic history into the political narrative, this chapter illustrates well the value of the book as an introductory text for students and others who need a comprehensive but comprehensible history of Southeastern Europe. The whole book deals lucidly with a subject of great complexity.

Since so much material is given, it would be churlish to complain that important subjects are dispatched in a few sentences, or that the book offers so much solid information as to be almost indigestible. Better to say that it offers fare for many historical meals and whets the appetite for more. It is also well-garnished with clear and simple maps illustrating each section. There are useful, if selective, suggestions for further reading and an adequate index. It will be a difficult book to supersede but should pave the way for new thematic studies in the same field.

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K. A. DIMADIS, editor. *Balkan Bibliography. Volume IV-1975*. In two parts. Thessaloniki: Institute for Balkan Studies. 1977. Pp. xliii, 459; 335.

This volume, in two parts, is one more in a series begun several years ago. The first part contains a bibliography of 4,178 numbered entries of works published in various languages, mostly during 1975. These works deal with the Balkans primarily from later Byzantine times into the mid-twentieth century. The entries are divided into eight major sections, starting with general works and ending with history. Within each division there are further subdivisions and cross-references, given alphabetically by an author's name wherever appropriate, or by a title elsewhere. Each entry is initialed by either the editor, K. A. Dimadis, or by one of the thirteen others who worked with him on the compilation. A number of entries are accompanied by Greek annotations, whatever the original language of the cited work might have been. The history section is a long one, including sixty-one subdivisions such as, for example, Bulgaria from 1878 to 1917. Titles and authors' names are given in the original language; but there are also Greek translations for titles which did not originally appear in widely known languages like English, German, and French.

The second part, a supplement, has a very different organization. This is a mixture of various kinds of articles. Some are direct Greek translations of works appearing originally in languages such as Turkish, Albanian, or Russian. Others are Greek reviews of articles taken from non-Greek sources. Still others are original Greek studies. In places, too, there are English résumés following a Greek text. Finally, there is a short section of notices including an English-language description of classical Greek studies in Japan.

As the types of articles vary in this volume, so also do the topics and lengths. A page-and-a-half Greek translation of a Turkish article on the derivation of the word "Turk" contrasts with a ten-page review in Greek of a German work dealing with Greek diplomacy during the period 1919-20, while the Greek translation of a Russian article on John Capodistrias's life in Russia takes twenty-nine pages.

Least successful seem to be the English résumés of the Greek texts in the supplement. They are not quite as informative as they might be, the language is sometimes stilted, and a few sentences are in obvious need of editorial revision. All in all, however, this is a minor matter in a series written primarily, and very adequately, for a Greek-speaking audience. Yet it should be emphasized here that the arrangement of materials is such that even those scholars without a knowledge of Greek will, with a little effort, find this work—and most particularly the first of the two parts under review—a useful reference tool.

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LUCIAN BOIA. *Relationships between Romanians, Czechs and Slovaks, 1848-1914*. Translated by SANDA MIHĂILESCU. (Bibliotheca Historica Romaniae, number 54.) Bucharest: Editura Academiei Republicii Socialiste România. 1977. Pp. 157. 7.50 L.

Nineteenth-century attempts at cooperation by the non-German and the non-Magyar peoples of the Habsburg monarchy to protect their national interests have received little scholarly attention. Perhaps the reason is that these attempts were sporadic and had little effect on the course of events. Lucian Boia's work, the first monograph on the relations between the Romanians of Transylvania and the Czechs and between the Romanians and the Slovaks, tends to confirm that observation. Although this book is in a sense a study of failure, it nonetheless provides valuable insights into the nature of the national movements in the monarchy.

The author describes the tenuous relations of the

Czechs and Slovaks with the Romanians on the basis of an extensive bibliography of printed books and articles in Romanian, Czech, and Slovak, the contemporary newspaper press, and materials from Romanian and Czechoslovak archives, contacts were primarily cultural and between individuals until the 1890s. Characteristic was the visit of the Czech philologist Jan Urban Jarník to Transylvania in 1879, which resulted in the publication of a valuable collection of Romanian folktales. Other forms of cooperation between Romanians and Czechs were difficult, owing to the absence of common interests; the Czech lands belonged to the Austrian half of the Monarchy, while Transylvania had been incorporated into Hungary. The visit of a Romanian delegation to the Prague International Exposition of 1891, for example, had no lasting result.

A strong basis for Slovak-Romanian cooperation existed, since both peoples resided in Hungary and were objects of the same policy of Magyarization inaugurated by the Hungarian government in the 1870s. Yet even here successes were dishearteningly few. One bright spot was undoubtedly the work of the Slovak journalist, Gustav Augustiny, whose career is described in detail. Augustiny, who was an editor of two major Transylvanian Romanian newspapers between 1893 and 1900, served as an intermediary between Slovak and Romanian political leaders. He had much to do with organizing the so-called Congress of Nationalities in Budapest in 1895, which Boia considers the high point of Slovak-Romanian relations. A protest by the leaders of the two peoples and the Serbs of Hungary against limitations of national autonomy imposed by the Hungarian government, the congress was a desperate effort to attract the benevolent attention of Europe to their cause. The "alliance," however, foundered on the internal political instability of all three nationalities. Boia briefly describes the subsequent return to political "activism" of the three after the turn of the century, but the cooperation of their delegates in the Hungarian parliament was generally fruitless. In the years immediately preceding the outbreak of World War I the disenchanted leaders of the Slovaks, Romanians, and Serbs sought bilateral settlements with the Hungarian government.

Perhaps the most positive result of cooperation among the nationalities was that each gradually learned more about the others, thanks largely to a vigorous and alert press. Indeed, newspapers played a key role in the respective national movements. They were not only purveyors of information, but also served as indispensable centers for the mobilization of political and cultural forces. In the final analysis, as Boia tacitly recognizes, cooperation among the nationalities failed because the

leaders of each were preoccupied with strictly local problems. As representatives of national traditions and aspirations they were parochial rather than internationalist in outlook.

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JOHN S. KOLIOPOULOS. *Greece and the British Connection, 1935-1941*. New York: Oxford University Press. 1977. Pp. xvii, 315. \$24.00.

In this book John S. Koliopoulos offers a comprehensive analysis of Anglo-Greek relations during the second half of the thirties. He effectively presents the primary determinants of events and provides important new information, primarily from British and Greek archival sources, regarding the principal considerations underlying Anglo-Greek relations and Britain's influence on Greece's internal and external affairs.

Although there was a considerable community of interest in the Anglo-Greek relationship, there was also a substantive divergence caused by conflicting strategic considerations. The expectations of Greece's leadership, on the one hand, proceeded from the assumption that British interests in the eastern Mediterranean would ensure British support for Greece in the event of war. This implied an overestimation by the Greek leadership of their country's strategic value, which, in so far as Britain was concerned, was almost negligible at that time. Moreover, the Greek General Staff's strategy and preparations, which envisaged only a defensive, static war primarily directed against Bulgaria, further diminished Greece's strategic importance. While Italy's aims in the Balkans and the eastern Mediterranean were always a matter of concern to Greek leaders, they never contemplated a war with Italy through Greece's Albanian frontier, much less a conflict with Germany. On the other hand, the Italian factor to a large extent determined Britain's policy toward Greece. The British believed that they could enjoy the benefits of an alliance with Greece without committing land forces to its defense. Thus, the repeated efforts of the Greek leaders to create a formal, reciprocal alliance met with failure. Britain's policy remained practically unaltered even after the Italian attack on Greece. The defense of Crete, a base of major strategic importance, was the most the British ever contemplated. And Britain concentrated its efforts on preventing a separate Italo-Greek peace.

The imminence of a German drive into Greece, however, forced Britain to reverse its policy and help Greece with land forces, a decision based on

the remote possibility of inducing Yugoslavia and Turkey to join Greece against the Axis. But political considerations could hardly be reconciled with military realities. From the military point of view, Britain's belated decision to help Greece was unsound, and the "allocation of forces . . . made it almost certain that the British and the Greeks would lose everything except their honour" (p. 258). Furthermore, in anticipation of a German attack the Greeks did not really want the kind of help England was able to provide, for "if Greece had to fight Germany, her Government was resolved to fight for a quick and honourable defeat" (p. 300). But, if such an "honourable defeat" was looked upon by most Greeks as an inescapable evil, the same cannot be said for at least some of the military leaders, who contributed to the disastrous disintegration of the Greek forces in April 1941, through defeatism, disobedience, and treason.

This study focuses primarily on foreign policy, but it also gives some attention to Greek internal affairs. The author's rather cursory treatment of domestic issues and his occasional hesitation in grappling with certain controversial problems result in a not altogether satisfactory analysis of sociopolitical determinants. This reservation notwithstanding, this is the most authoritative treatment of the subject and an important contribution to modern Greek historiography. A more complete story cannot be known, of course, until all of the Greek archival sources become available; even then lacunae will remain because important sources have been destroyed.

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MUSTAFA IMAMOVIĆ. *Pravni položaj i unutrašnje-politički razvitak Bosne i Hercegovine od 1878. do 1914.* [The Legal Status and Internal Political Development of Bosnia and Herzegovina from 1878 to 1914]. Sarajevo: "Svjetlost" Izdavačko preduzeće. 1976. Pp. 274.

The history of internal politics in Bosnia and Herzegovina during Austro-Hungarian rule (1878-1918) has suffered scholarly neglect because of the shadow of the sensational assassination of the Habsburg Archduke Franz Ferdinand at Sarajevo in 1914. While millions of words have been written about the conspirators' plot to kill the heir apparent and their putative links with the Belgrade government, historians have generally overlooked the more conventional political movements that dominated public life in the provinces until the assassination. Mustafa Imamović, a Yugoslav legal historian at the University of Sarajevo, has helped fill

this gap with an excellent treatment of the major ethnic political organizations that were formed during the Dual Monarchy's rule.

Imamović argues that political life in Bosnia and Herzegovina was heavily influenced by the ambivalent legal and constitutional conditions of Habsburg administration. The Dual Monarchy introduced many modern administrative services and sought to develop the region industrially, but national and social conflicts significantly weakened the government's capacity to implement decisive reforms. The legal status of the provinces remained cloudy until the monarchy annexed the area outright in 1908. Torn between Austrian and Hungarian interests, civil servants from the monarchy were unable to address basic ethnic and social conflicts in Bosnian society. Benjamin von Kállay, who headed the Bosnian administration from 1882 to 1903, promoted the notion of a distinct Bosnian identity to counter the threat of ethnic separatism by Serbs, Muslims, and Croats. His program failed. Members of all three groups formed confessionally based movements even though Kállay refused to recognize the right of confessional groups to organize politically.

After Kállay's death in 1903 his successors permitted the local inhabitants to conduct political activities. All three ethnic groups formed "national organizations" between 1903 and the first meeting of the Bosnian *Sabor* (Parliament) in 1910. Imamović deals in detail with the social foundations and political program of each organization. He contends that the leaders of national organizations, by favoring ethnic and confessional solidarity in their relations with the monarchy's authorities, insured that public life would be dominated by ethnic rather than social issues for the duration of Habsburg rule.

This book is based principally on thorough research into published sources, particularly the contemporary press. Imamović also draws upon some archival sources and upon a thorough knowledge of the Western literature on relevant problems. His work is a clear and objective presentation of much unexplored material.

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GERSON S. SHER. *Praxis: Marxist Criticism and Dissent in Socialist Yugoslavia.* Bloomington: Indiana University Press. 1977. Pp. xix, 360. \$15.00.

The Praxis circle and its journal and conferences represent what surely must be one of the most sympathetic and creative efforts by a group of university-based Yugoslav intellectuals to create a

viable and democratic Marxism. In this effort, they also attempted to function as a leavening element in the evolution of the Yugoslav League of Communists and Yugoslav official Marxism on the one hand, and of the general European Marxist milieu on the other. While academic in form and thrust, the Praxis circle was also intensely political in the issues to which it addressed itself and in the manner in which it focused its debate. It, therefore, had an impact on West European Marxists in and outside of the movement, as well as on East European Marxist humanist revisionists, particularly in Poland, Hungary, and Czechoslovakia.

Gerson S. Sher's book is the best available source in any language on the history and evolution of this unique group. He clearly made this a labor of love, spending considerable time in Yugoslavia at the summer schools on Korčula, and in discussions with various editors and participants in the Praxis circle. He has also read almost everything that is available. Consequently, his book—whatever its defects—will remain the standard reference for some time to come.

If there are weaknesses in the work, they come from an overly narrow concentration on the Praxis circle itself and its view of the social and political reality of Yugoslavia in the 1960s and early 1970s. He is relatively brief in his discussion of the broader milieu in Yugoslavia, which made the phenomenon of Praxis possible, and which placed substantial obstacles in the way of the crackdown on this group when it did occur. For Soviet and East European specialists, for example, the fact that it took six long years for the party, or rather the League of Communists, to cause *Praxis* to cease publication after endless wrangling in courts and in university and cultural institutions would be an indication of the degree to which Yugoslavia has evolved away from a command society, rather than an indication of repressiveness. Even the repression that occurred was, by the standards of one-party states, very mild indeed. The *Praxis* editors from Belgrade were, after a prolonged struggle, barred from teaching, but they continued to collect their professorial salaries and to travel abroad to lecture at foreign universities.

The point is, of course, that what made Praxis a significant current was the fact that it was the tip of an iceberg in Yugoslav Marxism, expressing more clearly and intransigently views which were widespread within the League of Communists and even within its leadership. Far from being an isolated current in the academy, for a number of years the Praxis group was dominant in the fields of philosophy and sociology. It dominated the faculty of philosophy in Belgrade, the largest liberal arts school in Yugoslavia, and the departments of philosophy and sociology in Zagreb, the second

largest in the country. This was not an accident but the reflection of the organic link of the Praxis circle members with the mainstream of Yugoslav Marxism, within which they evolved and the basic framework of which they never abandoned.

The Praxis circle had an impact well beyond Yugoslavia, particularly in the mid- and late-1960s, when its summer school and international board of editors read like a Who's Who of the nondogmatic and non-Stalinist European Marxism that was beginning to re-emerge after a hiatus of several decades. Happily, much of the debate is accessible to non-Yugoslav scholars in the international editions of *Praxis* and in the numerous books published in English, French, and German by the leading members of the circle, Rudi Supek, Mihailo Marković, Sveta Stojanović, Ljubo Tadić, and others. Their long-range impact on Yugoslav philosophy, social science, and political dialogue itself is more difficult to assess, since the processes with which they interacted and the issues which they have raised remain relevant today and will certainly be relevant in a post-Tito Yugoslavia. This is, for all the reservations, an excellent and worthwhile book which should be read by any scholars interested in contemporary Marxism, the fate of Yugoslavia, and the problems of one-party polities.

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MARIAN RYBICKI, editor. *Sejm Ustawodawczy Rzeczypospolitej Polskiej, 1947-1952* [The Legislative Assembly of the Polish Republic]. Wrocław: Osolineum. 1977. Pp. 349. 80 Zł.

The need for monographs presenting the story of the postwar national legislature in Poland is obvious. The thirty-plus years of the Sejm's existence have demonstrated many aspects of its unique position to the eyes not only of trained observers but of the general public as well. Unfortunately, the total sum of observations leads to a sad conclusion: while the Sejm is legally recognized as the "supreme organ of state authority," it must follow—like any other organ of the state—a higher "political authority" that is reserved for the Communist Party (Polish United Workers' Party) as the representative of the working people.

In post-Stalin discussions concerning a Polish model of "socialist democracy," the topic of the Sejm occupied a very significant place, capturing the interest of many scholars, politicians, and journalists alike. The attention paid by them to the Polish national legislature was especially marked during the periods associated with dramatic

changes of Poland's Communist leadership, the assumption of power by Władysław Gomułka in 1956 and by Edward Gierek in 1970. Books and articles that appeared at these times provided not only extensive interpretations of the beneficial effects of events on the Sejm but concrete proposals for the future as well. Without exception, the revival of the Sejm was welcomed as a positive development, and suggestions dealt with ways and means by which its newly won position could be strengthened and its role in the total political system further improved. But, as usually happens in Communist-ruled countries, the reformist goals of innovators made little impact on timid party leaders who were afraid to give the Sejm more independence lest it become a competitive political force. In time, the obstinacy of party bosses dampened the spirit of reformists who either stopped writing about the Sejm altogether or fell into a routine of strictly formalistic considerations. The treatise under review belongs to this unattractive category.

The book, whose topic is the 1947-52 period, consists of a short preface, seven chapters, and an annex. It is a joint effort of five authors, one of whom, Kazimierz Działocha, wrote chapters two, three, and six, over one-third of the whole. Chapter one by Marian Rybicki, editor of the book, is a description of the political situation in Poland before the infamous elections of 1947 and shortly thereafter. In a sense, it is the most interesting chapter of the entire volume, but the evident partisan presentation of facts and their one-sided interpretation makes it more a propaganda bit for the Communist cause than a truly objective piece of scholarship. One can say more or less the same of chapter seven, "The Organization and the Mode of Preparation and Passing of the PPR Constitution," by Andrzej Gwizdz. Chapters two through six inclusive might, perhaps, be interesting to constitutional lawyers, but behaviorist scholars and general readers will find them hard to swallow. On the whole, the world would not be worse without this book, and its undoubtedly industrious co-authors could better spend their time and effort on other things.

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A. A. ZIMIN. *Krupnaia feodal'naia votchina i sotsial'no-politicheskaia bor'ba v Rossii (konets XV-XVI v.)* [A Great Feudal Estate and the Social-Political Struggle in Russia (The Late Fifteenth Through the Sixteenth Centuries)]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 356. 1 r. 80 k.

This important new study of the Iosif Volokolamsk Monastery from its founding (1479) through the

sixteenth century should be of interest to all Russian historians. A. A. Zimin begins by accurately observing "the clear need to study the micro-structure of society from all sides" (p. 4). With this in mind it is unfortunate that he chose only "to study the social structure" of the monastery "in close connection with the social-political life of Russia" (p. 6). After the introduction, Zimin addresses five topics that form the principal subdivisions of the book: the monastery's archives, history, social composition, and landholdings, and the Iosifite doctrine. Had Zimin also examined the economic activities of this dynamic institution this work could be considered more definitive. Nonetheless, it does provide valuable and interesting insights, particularly into Muscovite society at the provincial level.

The book begins with the obligatory, but useful, historiographic introduction. Zimin next surveys the extant sources, providing a brief history of the monastery's archive. He points out that the Volokolamsk Monastery is one of the only institutions for which acts, manuscripts, and financial records have all survived relatively intact. Although Zimin makes wide use of both published (he himself edited the most important of these) and archival materials of the first two types, he virtually ignores what he describes as rich records concerning the monastery's finances. Despite the fact that Zimin remains close to the sources, he often reads more into them than caution would deem prudent and fails to fully utilize available material. In his conclusion, for example, Zimin traces Iosifite influence at the court of Ivan IV simply by recounting the rise and fall of its principal supporters within the Church hierarchy. Surely factors other than Ivan's current attitude to this group affected the careers of its members during those turbulent times. The author's conclusions would have benefited from additional evidence such as immunities or gifts to the monastery.

Zimin's history of the monastery in its early years provides excellent insight into the political maneuverings of the appanage princes in their last efforts to resist the encroachment of Moscow. But can the late 1470s accurately be described as the "concluding stage in the struggle for political predominance in Rus' between Moscow and Tver'" (p. 52)? By the sixteenth century the Volokolamsk Monastery had become the leading monastic institution, and Iosif and his successors are clearly shown to be adept at exploiting the opportunities created by these times.

Zimin is strongest in his examination of landholding and the social composition of the monastery's brethren, servitors, and benefactors. These studies are particularly valuable since little research has been done on Muscovite society at the

local level. Zimin shows that the monastery drew its members and support chiefly from middle-level landholders in the surrounding areas. They provided most of its lands (usually when they had no heir) and income. Although appanage princes also gave the monastery large amounts of land during its early years, gifts from the grand princes were almost exclusively monetary. The value of these sections is somewhat diminished by Zimin's style. He presents copious accounts of families and needlessly long individual biographies. These are clearly the best documented examples and not random illustrations. One wonders how representative such cases actually are. Summaries of all available data along with an evaluation of the completeness of the sources would have been more useful.

Zimin's treatment of the ideological history of the Iosifites is his least successful section. The subject has been dealt with more thoroughly elsewhere (Budovnits, Kazakova, and Lur'e), and Zimin does little more than rehash traditional interpretations. Nevertheless, it can provide a useful introduction to the subject.

Despite its weaknesses this book makes an important contribution to the understanding of this period. It gives valuable new insights into social and political dynamics at the local level as formerly independent areas were absorbed by the growing Muscovite colossus. If this work serves as a stimulus for further in-depth regional studies, its significance will have been great indeed.

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I. A. BULYGIN. *Monastyrskie krest'iane Rossii v pervoi chetverti XVIII veka* [Monastic Peasants of Russia in the First Quarter of the Eighteenth Century]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 326. 1 r. 90 k.

Soviet historiography has generally neglected the Church in imperial Russia, but it has given some attention to the economic dimension—church lands and peasants. Usually that means examination of monastic property and peasants, the forms of "feudal rent," and the saga of peasant revolt. To that tradition belongs this new volume by I. A. Bulygin, a study of monastic peasants during the Petrine reforms. Here the author has made a count of monastic peasants, examined Peter's policy toward monastic properties, calculated peasant dues, and tabulated monastic revenues. Besides the familiar printed sources and extensive pre-revolutionary literature, Bulygin has utilized some new archival materials—primarily the *fondy* of

three monasteries, the Synod, and the *Monastyrskii prikaz*.

The result is a rather narrow and pedestrian study, yet useful in several respects. Of some value is the careful retabulation of data on monastic peasantry and its distribution. The data, while confirming the Soviet thesis of a population increase during the Petrine reforms (*contra* Miliukov), also show that the rate of growth did decline sharply and that in some areas the population declined absolutely. The author also presents a close analysis of Peter's "secularization," stressing the permanence and significance of the reforms. *En passant* Bulygin makes the important observation that the prevailing picture of the Synod as a pliant tool of the state requires revision. In lengthy chapters on peasant dues Bulygin shows that, unlike the state (which consistently converted all dues into monetary levies), monasteries pursued a more variable policy—determined primarily by the legal status of a monastery and its lands (*opredelemnnyi*, *neopredelemnnyi*, *zaopredelemnnyi*—terms to which Bulygin gives new meaning and precision). Finally, the author uses the archive of Pafnut'ev-Borovskii monastery to draw a graphic picture of the monastic economy and its development during the Petrine reforms.

The monograph suffers, however, from serious deficiencies. Above all, it is wedged tightly within the framework of traditional *problematika*; it does not explore new questions, new approaches, new methodologies (the mathematics, for example, not rising above tabulation). On such old problems as secularization the author does not offer major new insights or new sources and, indeed, fails to use any pertinent Western literature (for example, works by Cracraft, Smolitsch, and Wittram). Finally, though emphatic about the significance of Peter's secularization, Bulygin does not examine the effect upon the Church, its politics, its administration, and its social functions.

Still, as a statistical compendium, as a survey of Petrine policy, and as an account of levies imposed on monastic peasants, Bulygin's volume will be of some use to institutional and economic historians of Petrine Russia.

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A. M. STANISLAVSKAIA. *Rossiiia i Gretsiiia v kontse XVIII-nachale XIX veka: Politika Rossii v Ionicheskoj respublike, 1798–1807 gg.* [Russia and Greece in the Late Eighteenth and Early Nineteenth Centuries: Russian Policy in the Ionian Republic, 1798–1807]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 373. 1 r. 89 k.

Avgusta Mikhailovna Stanislavskaia, a Balkan historian at the Academy of Sciences' Institute of History in Moscow, examines Russian diplomatic initiatives in the eastern Mediterranean during the later eighteenth and early nineteenth centuries by documenting Russian involvement in the Ionian Islands in the period immediately following Napoleonic occupation of the islands.

The basic chronology of the Ionian campaigns has long been available to Western audiences, most recently in Norman Saul's account, *Russia and the Mediterranean, 1797-1807*. Stanislavskaia's work provides a recital of the dramatic events—the Ushakov expedition and defeat of the French in 1798-99, the disorders and dissensions among local islanders, and the attempts to establish a viable constitutional government on the islands, until the ultimate demise of the Ionian Republic in the wake of the Russo-Turkish War (beginning in late 1806) and the Tilsit Agreement of 1807.

Stanislavskaia uses previously untapped diplomatic correspondence from the files of the Archive of Russian Foreign Policy, especially the collections of the "Chancellory," "Russian Relations with Turkey," the "Russian Embassy in Constantinople," and "Russian Relations with the Ionian Islands." In the process she inadvertently demonstrates how very limited in scope is the current Soviet publication project of diplomatic papers, *Vneshniaia politika Rossii XIX i nachala XX veka: Dokumenty rossiiskogo ministerstva inostrannykh del* [Russian Foreign Policy of the Nineteenth and Beginning of the Twentieth Centuries: Documents of the Russian Ministry of Foreign Affairs].

Stanislavskaia contends that the Russians responded positively to the aspirations of Ionian middle-class elements, long subjugated by the islands' native aristocracy, while also identifying with the wider Greek national awakening. Russian aid for a nascent liberal bourgeoisie and identification with early national movements in the Balkans are familiar ideas in Soviet historiography. In this case, the thesis has some limited applicability. Stanislavskaia traces the liberal provisions of the 1803 Ionian Constitution to the constitutional tendencies of Alexander I's Unofficial Committee, particularly to the views of Adam Czartoryski on the Greek question. G. D. Mochenigo, the Russian plenipotentiary on the islands, and the young Greek islander, later Russian foreign minister, John Capodistrias, are viewed as having similar predispositions to those of the Unofficial Committee. It is a disputed question, however, whether local islanders perceived the Russian protectorate as embodying liberal reform.

On the national question, Stanislavskaia holds that the corps of Greek infantrymen, formed out of the nucleus of the Odessa Greek battalion between

1803 and 1805 for use in the Mediterranean, became part of a much broader Russian identification with the national aspirations of the Greeks. Stanislavskaia disregards the current challenge to traditional nationalist interpretations of early nineteenth-century Greek movements. Her documentation regarding Grecophile motives at court is nevertheless of interest to historians of modern Greece.

Behind Stanislavskaia's monograph lies the much larger issue of Russia's eastern policy at the outset of the nineteenth century. Stanislavskaia holds that the Russian government, in the first years of Alexander I's reign, consciously supported the development of Greek national feeling. This thesis could be advanced more persuasively by appealing to the continuing economic and religious ties of the Russian state with local Greek Orthodox institutions—a point exemplified in this period in the symbolic gesture (not mentioned by Stanislavskaia) of imperial patronage of the holy relics of St. Spyridon, patron saint of Corfu. Still, it is probable that, the hopes of prominent Greeks and Grecophiles in Russian service notwithstanding, Alexander I was more preoccupied with playing off French and Ottoman opponents than he was in fostering any "premature" (to use Capodistrias's expression) Greek liberation movement.

Followers of Stanislavskaia's interesting research will note the reference (p. 57) to her next book, a study of the activity of Admiral F. F. Ushakov in Greece. Perhaps that work will contain a separate bibliography, which this volume does not.

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W. BRUCE LINCOLN. *Nikolai Miliutin: An Enlightened Russian Bureaucrat*. (Russian Biography Series, number 3.) Newtonville, Mass.: Oriental Research Partners. 1977. Pp. ix, 130. Cloth \$9.00, paper \$4.50.

Nikolai Miliutin was one of the most important statesmen of nineteenth-century Russia, yet he has never been the subject of a scholarly biography. W. Bruce Lincoln has worked in Soviet archives as much as any American historian, and his many articles are much admired by specialists on Russian political history. It would seem that this book represents an ideal conjunction of author and subject. An incompatibility between matter and format, however, will leave many readers disappointed.

The book is divided into nine chapters, each devoted to a stage of Miliutin's career. Almost all references in four of these chapters are to archival

sources, evidence of Lincoln's erudition and thoroughness. Yet in a brief popular biography, it is almost impossible to use this material to best effect. Specialists will want more details on Miliutin's memorandum of 1841 on railroad construction and his note of 1847 on the reform of serfdom. The general reader, on the other hand, will be put off by Lincoln's tendency to refer him to particular monographs, curtailing his own discussion of many issues in which Miliutin was involved. Furthermore, the evidence is still deficient for some important themes, despite Lincoln's searching. Among these is the crucial question of Miliutin's associations in the decade prior to his rise to prominence in 1858. Here Lincoln must rely on inference and on the questionable testimony of autobiographies. Yet when the sources are few and fragmentary, it is hard for the historian to be succinct.

Finally, elucidation of Lincoln's central conception, the "enlightened bureaucrat," was apparently truncated to save space. Miliutin was not quite the ideal Weberian bureaucrat; he acquired much of his education and all of his training on the job, and, as Lincoln correctly emphasizes, he gradually lost faith in purely bureaucratic remedies. At first, Lincoln simply contrasts the enlightened bureaucrats to the venal and indolent majority of officials. Later, he emphasizes the confluence and reciprocal influence of reformers within the administration and the moderate intelligentsia. Throughout, he stresses the enlightened bureaucrats' insistence that the state must stand above castes and classes, and he is prone to give them credit, at least implicitly, for achieving that stance themselves. For a more solid understanding of enlightened bureaucrats, however, and also for a lucid discussion of many related matters, one must turn to Lincoln's articles. Better editing would have improved this work, but Miliutin's career and the issues it raises do not lend themselves to satisfactory treatment in so short a book.

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DIETRICH GEYER. *Der russische Imperialismus: Studien über den Zusammenhang von innerer und auswärtiger Politik, 1860-1914*. (Kritische Studien zur Geschichtswissenschaft, number 27.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 344. DM 54.

This important, tightly packed book examines the chief features of Russian imperialism, 1860-1914, probing more deeply than previous works for Russian interactions between foreign policy and domestic components such as finance, economic development, the regime, and public opinion.

Eschewing startling new departures or a detailed description of Russian expansion, Dietrich Geyer analyzes the basic problems of Russian imperialism thoroughly and with enormous erudition.

In the first of three weighty chapters, "Reform and Expansion, 1860-1885," Geyer reaffirms Alfred Rieber's argument in *The Politics of Autocracy* (1966) that the emancipation was undertaken primarily as a "precondition for strengthening military striking power . . ." (p. 21). He emphasizes the disarray and confusion of the reform era, financial weakness, and economic underdevelopment as hindrances to an imperialist policy. What, then, prompted Russian adventures in the Balkans and Central Asia? The answer lies partly in a socially based, compensatory nationalism, fostered by the regime to buttress its uncertain prestige. In the Russo-Turkish War of 1877-78, St. Petersburg yielded to massive pan-Slav pressure in order to reinforce its authority with the Russian public, and then lost any advantages by backing down at Berlin because of military and financial weakness. The resulting public disillusionment revealed how swiftly loss of prestige abroad could erode the regime's credibility at home, an erosion fostering the success of the terrorist "People's Will."

For Central Asia Geyer cites the relevance of Schumpeter's thesis of imperialism as atavism and rejects Soviet claims that economic factors were primary in expansion: Russia's Asian policies were dependent on European power politics. When Anglo-Russian rivalry in 1885 endangered Russia's crucial European interests, St. Petersburg sought peaceful compromise. Russia's aggressive policies in the Balkans and Central Asia, Geyer concludes, failed to stabilize the regime, drew Russia deeper into dangerous imperialistic competition, and imposed new burdens on a shaky autocracy. That the nationalistic Alexander III brought Russia into the second *Dreikaiserbund* in 1881 merely revealed the lack of realistic alternatives for a helpless Russia.

In "Borrowed Imperialism, 1885-1905" Geyer emphasizes, perhaps excessively, negative factors of crisis, weakness, and near bankruptcy. (After all, tsarist Russia survived and made rapid economic progress in this era!) While praising Witte, Geyer concludes that Witte fell victim to his own bold efforts to industrialize Russia, and that the costs of his industrial system were revealed in the political crisis of 1904-05. Indeed, Witte's program of far-reaching economic imperialism, especially "peaceful penetration" of Manchuria, Geyer concludes, provoked conflict with Japan. Analyzing the relationship between the Russo-Japanese War and the 1905 Revolution, he notes that the Treaty of Portsmouth failed to halt or even dampen Russia's internal crisis.

In "Between Wars, 1905–1914" Geyer finds the constitutional monarchy unable to implement basic reform or escape a traditional, nationalist, Balkan-centered foreign policy. The leadership believed in an inevitable clash with the German powers but sought to delay it while Russia rearmed. Geyer remains unconvinced by Soviet allegations that Russia in 1914 faced a "revolutionary situation" since there was no accumulation of discontent like that of 1904. A nationalist great power ideology held the old society together until both succumbed to military defeat. Meanwhile, the Russian colonial regime in Asia, designed to serve the *métropole*, failed to enlist public support or to seek solutions to the perilous nationality question.

There are few flaws in this impressive work, but a summary would have been helpful, especially since Geyer's complex style will prove almost impenetrable to non-German readers. The notes contain a gold mine of bibliographical data interspersed with succinct commentary. Scholars concerned with imperialism and Russian domestic and foreign problems will welcome this thought-provoking work.

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RICHARD STITES. *The Women's Liberation Movement in Russia: Feminism, Nihilism, and Bolshevism, 1860–1930*. Princeton: Princeton University Press. 1978. Pp. xx, 464. Cloth \$37.50, paper \$12.50.

Two years after the Bolshevik Revolution of 1917 Lenin proclaimed that the new Soviet government had already done more to advance the position of women than had been accomplished by all democratic republics in the previous two centuries. Though somewhat extravagant, the claim was defensible, and the liberated women in the USSR have served as a model to feminists throughout the world ever since. Until quite recently, however, the historical background that helped shape the Soviet model has remained unexplored. For the past decade Richard Stites has been on the cutting edge of scholarship moving into this little-known field. In this account—drawn from a wide range of published sources, archival materials, memoirs, interviews, and personal observations—he traces the development of the women's movement in pre-revolutionary Russia, describes changes introduced by the revolution, and assesses gains achieved. Informative, richly documented, and eminently readable, the book synthesizes the author's pioneering work in several areas of investigation, and builds skillfully on the findings of other researchers. It offers photographic portraits, an index, and an excellent bibliography.

A brief introduction takes note of Russian traditions concerning women and of the influence of Western theorists on Russian social thought in the first half of the nineteenth century. Part two traces the emergence of the "woman question" and of responses to it (feminism, nihilism, and radicalism) in the post-Crimean reform period from 1855 to 1881. The next, longest, and most revealing part of the book describes the alternative paths to liberation taken by feminists and socialist women in the years up to 1917. The final major division, "Women's Liberation," deals with Soviet legislative and institutional innovation, the "sexual revolution," and postrevolutionary developments. The volume as a whole unfolds a complex tale of social dynamics, of competing and often contradictory goals, and of impressive achievements alongside persistent problems.

Stites clearly establishes the existence of a Russian feminist movement, something which had been called into question by some writers. Stites concedes that a movement as such arose only in 1905, but traces its lineage to earlier traditions of feminism in the country. A reform-minded contingent of Russian feminists—genteel, socially conscious, but concerned for the most part with the interests of the female intelligentsia—arose as early as the 1860s. Feminist energies were invested primarily at first in the cause of education for women and contributed to marked progress in this direction before 1917. Alongside the feminists, but surveying the social scene rather differently through their favorite blue spectacles, appeared a rank of female radicals. Although many were from the same social class as the feminists, these women were convinced of the need for political activity and played an important role in the revolutionary underground. While the feminist reformers gave priority to women's issues, the radicals (including the socialist women) subordinated women's specific concerns to more general political goals.

It was the "mystique of the franchise" that drew the feminists into political activity and catalyzed the incipient women's movement in 1905. But efforts to enlist the support of the Duma in the struggle for suffrage proved unsuccessful. It took the triumph of the revolutionaries in 1917 to bring the vote, along with a panoply of new rights, to Soviet women. Despite the Bolsheviks' genuine concern for women's rights, Stites argues, feminism—with its focus on the sexual division rather than the class divisions of society—was ideologically incompatible with Marxism and tactically unacceptable to the Bolsheviks. Women's interests continue to be subordinated to the larger interests of Soviet society—as interpreted by *men* who still retain political and economic control. The view that liberation means economic independence for

women accorded conveniently with the new regime's need to mobilize labor power and made it easier to move women into the work force. Under prevailing circumstances, however, this created new inequities. Stites writes sympathetically of the "double burden" of Soviet women today, yet appears to subscribe fully to the proposition that work outside the home is a necessary (if insufficient) condition of women's emancipation—an assumption that even some Soviet social scientists would challenge.

One of the noteworthy contributions of the work is its extended discussion of social attitudes toward sex and of sexual mores. The sexual revolution has a social history of its own that deserves attention, and Stites is one of the first to deal with it systematically. In discussing the recalcitrant problem of prostitution, the author reveals a sensitive concern with the exploitive nature of "white slavery." The result is a vivid scenario of female victimization that may, however, oversimplify the problem in both its psychological and economic dimensions. But here, as elsewhere, the work provides an invaluable springboard for reflection. This competent study promises to hold the interest of the general reader, reward the specialist, and open new perspectives in comparative women's studies.

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RICHARD HENNESSEY. *The Agrarian Question in Russia, 1905-1907: The Inception of the Stolypin Reform.* (Osteuropastudien des Hochschulen des Landes Hessen, series 2. Marburger Abhandlungen zur Geschichte und Kultur Osteuropas, number 16.) Giessen: Wilhelm Schmitz Verlag. 1977. Pp. 203. DM 38.

The thesis of Richard Hennessey's book is that the Russian government moved from a policy of agrarian reform in 1905 to one of peasant land reform in 1906. Under Witte's aegis in late 1905 and early 1906, the government's chief objective was to increase transfer of noble land to the peasantry by stepping up the Peasant Land Bank's activities. Under the new administration of Stolypin in the summer of 1906 the government changed course and concentrated on reorganizing landholdings within the peasant communes. The government's new goal was to make the peasant a hereditary owner of a consolidated parcel of land instead of temporary holder (in the case of the bulk of the peasantry who lived in repartitional communes) or hereditary owner of scattered strips of land.

According to Hennessey, the government embarked upon the new program first because the Witte program had failed. Nonpeasant speculators bought land offered by the Peasant Bank while

peasants did not buy this land because they believed expropriation of noble estates was imminent. Second, the government came to the conclusion, a view fostered by several noble congresses held during 1905 and 1906, that if the peasant became owner of his own piece of property he would respect noble estates, thus establishing law and order in the countryside.

Hennessey's examination of the Russian government's attempt to make the Peasant Land Bank more viable and his lucid description of the bank's financial operations—the bonds issued by the bank to enable it to purchase land and the debentures issued to reimburse noble sellers—are fascinating and a real contribution to the history of the period. We need more research on how institutions in Russia actually worked and, in particular, more information on their financial mechanism.

Hennessey's theory that Stolypin changed the focus of the reforms which bore his name may be valid, although S. M. Dubrovskii's *Stolypinskaya zemel'naya reforma* emphasizes that Stolypin implemented previously considered proposals. Hennessey's claim that the agrarian reform had to be altered because land purchased by the Peasant Bank was not getting to the right people is plausible. I disagree, however, with his implication that Stolypin concentrated on making the peasant a yeoman farmer rather than giving him more land mainly to make him respect private property and to respond to pressures from the nobility. Making the peasant a law-abiding citizen was one of Stolypin's goals but it was not entirely a goal developed to pacify the nobility nor was it his only goal. Stolypin had suggested in 1902 and again in 1905 that consolidated farmsteads in place of separated strips would result in more efficient peasant agriculture. Indeed, there is reason to believe that consolidation and enclosure, combined with agronomic assistance, loans and cooperatives for purchase of machinery and livestock, and continued purchase of additional land through the Peasant Land Bank (also part of the Stolypin agrarian program) would have benefited some peasants and led to the elimination of marginal farmers. In other words, simply giving the peasant more land was not the answer to the peasant problem.

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ALFRED ERICH SENN. *Nicholas Rubakin: A Life for Books.* (Russian Biography Series, number 1.) Newtonville, Mass.: Oriental Research Partners. 1977. Pp. 79. \$8.00.

In this brief biography of the Russian writer, bibliographer, and librarian, Nicholas Aleksandrovich

Rubakin (1862–1946), Alfred Erich Senn attempts to assess his influence on the reading public of Russia and, especially, his role in preparing Russian society for the revolutionary changes of the twentieth century. This is no easy task in view of the enormous range of Rubakin's activities.

The motivating force of Rubakin's career was his belief in the power of books as the efficacious instrument of enlightenment and social change. Prior to his emigration to Switzerland in 1907, he was best known as the author of popular books on science and natural history designed for a semi-educated or self-educated readership. Senn devotes relatively little space to a discussion of the impact of these popular works beyond pointing out that they were very widely read and that they carried a materialist and humanist message that Russian conservatives found clearly subversive.

Senn concentrates in some detail on the difficulties surrounding the production of *Sredi knig* [*Among Books*] (1911–15). The first annotated recommendatory bibliography in Russian, *Sredi knig* was designed to fill the needs of libraries, book-dealers, and the most discriminating readers, but in its prefaces and notes, no less than in its choice of books, the same progressive and revolutionary assumptions that had marked Rubakin's popular works are evident.

The most palpable expression of Rubakin's love of books was his own collection. In St. Petersburg and again in Baugy-sur-Clarens, he assembled enormous and valuable libraries. He lent books freely and carried on correspondence with thousands of borrowers. His libraries, moreover, acted like magnets to draw to him many of the best-known Russian radicals of the day. Senn clearly feels that through these personal contacts Rubakin was able to influence their intellectual development.

Senn has drawn his assessment of Rubakin from recent Soviet scholarship and from research in Rubakin's archives in Moscow. The notes and bibliography are complete, but the book lacks an index, a serious impediment to tracing Rubakin's myriad personal contacts.

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PETER KENEZ. *Civil War in South Russia, 1919–1920: The Defeat of the Whites*. Berkeley and Los Angeles: University of California Press, for the Hoover Institution on War, Revolution and Peace, Stanford, California. 1978. Pp. xviii, 378. \$17.50.

In this solid study of the later years of the Russian Civil War in the south, Peter Kenez argues that "the Whites lost the Civil War above all because

they failed to build those institutions which would enable them to administer the territories under their nominal rule" (p. xiii). There are, of course, many reasons why those institutions were not built, and the author analyzes these reasons in detail by carefully examining the work of the Volunteer Army and its leader, General Anton Denikin, the Cossacks, events in the Ukraine, intervention, defeats in battle, and the final struggle by Denikin's bitter rival and successor, General Baron Petr Wrangel.

Kenez believes that the most tragic and unnecessary reason for the Whites' failure was the contempt White generals had for political affairs. To these royalist remnants of the tsarist military system, politics were less than honorable, and politicians were subversive scoundrels. Socialists, liberals, federalists, conservatives—they all talked too much. The best way to deal with politics and politicians was to ignore them and get on with the fighting. Unfortunately, military success and political action were inseparable, for the civil war was a profoundly political struggle. How a man of Denikin's openness and intelligence could have clung so tenaciously to his simplistic determination to stand above politics still boggles the mind. Certainly, his reliance upon military force alone crippled his campaigns and contributed significantly to the Volunteer Army's destruction.

Good soldier Denikin, fighting the articulate and revolutionary Bolsheviks, also persistently refused to understand the need for immediate and extensive land and labor reforms, representative government, and successful appeals to the masses. Intelligent men attempted to persuade him of the need to speak to the people, to effect social change, and to establish representative government. To give him his due, he authorized and supported several feckless efforts in these directions but always compromised them—for military reasons or because he or some member of his retinue underestimated their importance.

There was another fundamental reason for Denikin's failure to devise and build acceptable new institutions for the society he wanted Russia to become. Kenez emphasizes the nearly fatal symbiotic relationship between Denikin and the Cossacks. In their native districts the Cossacks were excellent fighters, the backbone of the Volunteer Army; but they were almost childishly parochial, they lost their fighting spirit when the war took them away from the Kuban or the Don, and they were determined to preserve their class privileges at any cost. Cossack leaders squabbled endlessly for status and independence and resented the Russian officers. Denikin could not live with the Cossacks, nor could he live without them. This dilemma debilitated and finally exhausted his

capacity to lead the fight for an "All-Russian Union."

Denikin remains an enigma. He was a good man, a splendid example of von Clausewitz's resolute general, personally brave, humble, humane, and honest, loyal to the concept of a unified Mother Russia, but willing to accept the idea of representative government, if the civil war was won. The long list of his failures argues, however, that what the Whites needed was a ruthless, clever, and eloquent political leader prepared to use any device, make any public appeal, or promise any reform that would help him win. The Bolsheviks had the monopoly on such leaders, which, with their highly centralized command system, clearly stated ideological goals, and shorter supply lines, gave them the edge that led to victory.

Peter Kenz has written a thoughtful and valuable study of men who could not adapt to new conditions and who were beaten by other men who could and did.

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DENNIS J. DUNN. *The Catholic Church and the Soviet Government, 1939-1949*. (East European Monograph, number 30; Keston Book, number 10.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. viii, 267. \$17.00.

Acrimonious—that is the apt description Dennis J. Dunn provides for Soviet-Vatican relations from 1939 to 1949. Except for the Jews, no sizable Russian religious community suffered more than the Catholics in the 1940s. Dunn has no difficulty cataloguing sources for Soviet-Catholic antagonism: 1.) the irreconcilability of atheistic and theistic world views; 2.) Soviet suspicion of the Church's direction from abroad; 3.) Moscow's association of Catholicism with foreign enemies, first the Poles, later, fascist Italy and Germany; and 4.) the re-emergence of the ancient rivalry of Eastern and Western Christendom following Stalin's wartime truce with Russian Orthodoxy. Finally, by 1945 the Kremlin saw in the Uniates (Catholic in papal allegiance, Orthodox in ritual) a hindrance to the Sovietization of newly incorporated regions, even national separatism in the case of west Ukrainians.

Primarily, Dunn provides a discourse in church-state diplomacy, narrowly defined. Theology or congregational life impinge on the narrative only as they figure prominently in Soviet and Vatican charges and countercharges. Despite this restricted focus, the author deserves commendation for covering, as no one else has, the causes and course of rocky Soviet-Vatican relations in the

1940s, and the predicament of Eastern Europe's Catholics and Uniates traversing a most lethal decade.

Thorough and well researched, Dunn's volume, nevertheless, lacks interpretations that consistently convince. First, the volume overestimates the impact of any given papal initiative. Dunn states that a 1942 rumor of Soviet-Vatican detente "rocked" Europe (p. 88). Reaction more closely resembled a ripple, given the Continent's rapt attention to the course of fighting in Russia and Africa. Or in relating Pius XII's decision in February 1942 to establish diplomatic ties with Japan, the author claims the "ramifications . . . for Lithuania and Poland, which were relying upon Vatican influence with the Allies . . . were tragic" (p. 98). What remaining influence the pope may have lost by thus displeasing Hitler's enemies hardly accounts for the woes of Lithuania and Poland. Affairs there were simply beyond the pontiff's control.

More disturbing is Dunn's ready support for the pope's decision "to reject National Socialism less than Communism." The Vatican is pardoned for "mutely siding with the Reich" since "it chose the lesser of two evils, and, on that basis, its position was justified" (pp. 175-76). Those troubled by the debatable assertion that Nazism was less evil than Communism also will want to examine the possibility that Pius XII acted squarely on the basis of self-interest. Cannot one argue that he showed less hostility toward Hitler than toward Stalin because the former appeared to be killing and persecuting fewer Catholics than the latter? Upon noting that Hitler was responsible for killing more people than Stalin, at least during World War II, it is painful to recall the pope taking his own into account first and foremost. An even-handed condemnation of Stalin and Hitler probably would have made no appreciable difference in the course of the Sovietization of Eastern Europe. Moscow's agents arrested anti-Nazi priests of the resistance as well as clerical collaborators. But, for what it is worth, it might have made a considerable difference in historical estimations of the pontiff's moral stature.

Still, Dunn has given us more to commend than contend and his study is a useful addition to the literature.

MARK ELLIOTT
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TARAS HUNCZAK, editor, with the assistance of JOHN T. VON DER HEIDE. *The Ukraine, 1917-1921: A Study in Revolution*. (Harvard Ukrainian Research Institute, Monograph Series.) Cambridge, Mass.: Harvard Ukrainian Research Institute; distributed by Harvard University Press, Cambridge. 1977. Pp. viii, 424. \$15.00.

Since the publication in 1952 of John S. Reshetar, Jr.'s pioneering work *The Ukrainian Revolution, 1917-1920: A Study in Nationalism* (AHR, 58 [1953]: 923-24), several important scholarly monographs have appeared in English that have greatly clarified many complexities of the Ukrainian situation. Among the most important works are those by Arthur E. Adams, Jurij Borys, Oleh S. Pidhainy, Oleh S. Fedyshyn, and Iwan Majstrenko.

The present volume is definitely a worthy addition to these works. It differs from them in one respect. It is the product of several scholars rather than one. The work emerged out of a New York conference held to commemorate the fiftieth anniversary of the Ukrainian Revolution. It consists of fourteen chapters with a brief introduction by Richard Pipes that sets the stage for the emergence of the Ukrainian problem. The volume evolved either out of papers that were presented at the 1968 conference or out of special studies the editor solicited from scholars who had dealt with some aspects of the Ukrainian Revolution in their previous works.

Each chapter is an independent and well-researched scholarly entity and each stands on its own merit. Chapter one, by Wolodymyr Stojko, analyzes the attitudes of the leadership of the Provisional Government in Petrograd toward Ukrainian national aspirations. Chapter two, by Ihor Kamenetsky, reviews the activity as well as inactivity of the Ukrainian Central Rada and the role of historian M. S. Hrushevsky in these events. Chapter three, by Taras Hunczak, details German overthrow of the Rada and the subsequent misrule of the country by Hetman Pavlo Skoropadsky. Chapter four, by Martha Bohachevsky-Chomiak, discusses the critical events during the Directory (November 1918-May 1920). Chapter five, by Yaroslav Bilinsky, examines the tactics Russian Communists used in their take-over of the Ukraine. Chapter six, by Jurij Borys, summarizes the programs and activity of various political parties in the Ukraine before and during the revolution. Chapter seven, by John S. Reshetar, Jr., scrutinizes the role of the Russian Communist Party in the Ukrainian Revolution. Chapter eight, by Ivan L. Rudnytsky, explores the ideological antecedents for the Fourth Universal that formally severed the Ukraine's link with Russia. Chapter nine, by Bohdan R. Bociurkiw, investigates the Russo-Ukrainian conflict within the Orthodox Church and the activity of numerous churchmen and laymen during 1917. Chapter ten, by Arthur E. Adams, probes the nature of the social convulsion in the Ukrainian countryside following Skoropadsky's downfall. Chapter eleven, by Frank Sysyn, considers the background and ideas of the anarchist Nestor Makhno. Chapter twelve, by Oleh S. Fedyshyn,

studies the evolution of German policy towards the Ukraine during World War I. Chapter thirteen, by George A. Brinkley, surveys Allied policies vis-à-vis the Ukraine from 1917 to 1920. Chapter fourteen, by Constantine Warvariv, documents American official attitudes toward the Ukrainian national cause.

Because this volume, like most symposia, does not have a unifying thesis, there are unavoidable overlaps as well as omissions. These, however, are minor. They are compensated for by the high scholarly quality of each chapter. The final product is very sound, and everyone associated with the planning and production of this volume deserves thanks. A copy of this book belongs not only in every library but on the desk of every student of the Russian Revolution.

BASIL DMYTRYSHYN
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NEAR EAST

R. STEPHEN HUMPHREYS. *From Saladin to the Mongols: The Ayyubids of Damascus, 1193-1260*. Albany: State University of New York Press. 1977. Pp. 504. \$40.00.

There is still a genuine need in Western scholarship for the study of Islamic dynasties during the medieval period, and the work of R. Stephen Humphreys has met that need with regard to the Ayyūbid dynasty. *From Saladin to the Mongols* is a detailed account of the political and military history of the Ayyūbids, who ruled Egypt and Syria from 1193 to 1260. The monograph is based on a thorough knowledge of the relevant Arabic chronicles and the modern scholarship on the subject. It is, moreover, a natural sequence to the work of the author's mentor, Andrew Ehrenkreutz, who has recently published a biography of Saladin. Ehrenkreutz's work is an extreme revision of the traditional interpretation of Saladin as the paragon of Muslim virtue, which has been presented in the past by Hamilton Gibb and others. Given the cynicism of Ehrenkreutz on the one hand and the idealism of Gibb on the other, Humphreys offers, at the outset, a very judicious and persuasive evaluation of Saladin's leadership. Equally important is the author's attention to the rise of the Mamlūk corps to predominance in the Ayyūbid army. This development is interpreted as part of the increasing militarization of Ayyūbid society, which excluded civilian and religious leaders from the elite and weakened the army's dynastic loyalty. The author's analysis more fully explains the implantation of the Mamlūk sultanate in 1260.

Despite the considerable merits of this monograph, Humphreys proposes the thesis that a profound transformation of the structure of political life occurred in Egypt and Syria in the first half of the thirteenth century; he argues that a decisive change took place both in the territorial relations between Egypt and Syria and in the political goals of the Ayyūbid elite. This thesis is doubtful. On the first point, the Zangid and Ayyūbid periods were an interlude—a discontinuity in the customary relations between Egypt and Syria. Egyptian hegemony over Syria-Palestine may be traced in the Islamic era from the reign of Ibn Ṭūlūn through the Fāṭimid period. Concerning the second point, it is very questionable whether there was a change in political thinking, hence the organization of the region, from conscious decentralization to unitary rule; the goal was consistently one of political unity despite the demands of dynastic rule.

The author's thesis results from his focus on Syria-Palestine, which emphasizes the political fragmentation of the period; from this point of view, one sees the Ayyūbid empire as a confederation of local principalities with interminable warfare among its princes. The focus on the principality of Damascus, specifically, obscures rather than highlights the general political evolution of the Ayyūbid empire that was taking place most clearly and most significantly in Egypt. The author's chronicle of events concerning Damascus does not, in this reader's opinion, "accurately reflect the constitutional evolution of the empire as a whole" (p. 13).

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THOMAS NAFF and ROGER OWEN, editors. *Studies in Eighteenth Century Islamic History*. (Papers on Islamic History, number 4.) Carbondale: Southern Illinois University Press. 1977. Pp. xii, 450. \$12.50.

This book is an edited collection of papers presented at a colloquium on eighteenth-century Islamic (read "Middle Eastern") history at the University of Pennsylvania in 1971. There are sixteen papers, arranged in three sections. Each section has a general introduction; the introductions are thoughtful and deserve careful reading.

The papers themselves are of uneven quality and cover only selected regions of the Middle East. The theme of the first section, entitled "The Central Administration, the Provinces, and External Relations," is established by Halil İnalcık's discussion of decentralization as the political mode of the eighteenth-century Ottoman Empire; clearly re-

lated to it are Andrew C. Hess's impressive reviews of relations between the North African Ottoman provinces and Istanbul, and Abdul-Karim Rafeq's workmanlike description of developing autonomy in the province of Syria. The section is filled out by an interesting survey of patterns in eighteenth-century Ottoman-European diplomacy by Thomas Naff, a review of Iranian internal politics and bureaucratic decline in the eighteenth century by A. K. S. Lambton, and a rather loosely put together and disappointing discussion by Norman Itzkowitz of *ulema* and political ideas held in eighteenth-century Ottoman circles.

The second section, "Resources, Population and Wealth," is introduced by Roger Owen, who summarizes what is known of socioeconomic trends of the seventeenth and eighteenth centuries—and it is precious little. Whatever one may think of the orthodox Marxist model, B. Cvetkova's description of developments in land tenure and class in the Balkans (and to some extent Anatolia) between the sixteenth and eighteenth centuries reflects an impressive amount of detailed research. The English précis, however, should be used carefully: *celeps* are not "minor tax collectors" (p. 168), but rather cattle dealers; they were important in the development of the food supply system, not in "revitalizing" state territories (pp. 169, 183). André Raymond's paper on urban mercantile wealth in Cairo is, like Cvetkova's paper, a summary of the author's previous publications; little new is added. His paper is paralleled by an analysis of the sources of wealth of the learned aristocracy in eighteenth-century Cairo by Afaf Lutfi al Sayyid Marsot. The section is rounded out by Robert Mantran's general summary of eastern Mediterranean trade, a wryly entertaining review of what little is known of population figures in the century with some notes on natural resources by Charles Issawi, and a curious methodological article by Brian Spooner on the relationship between "the desert and the sown" in Iran which, lacking any time reference, does not belong in the book.

The third and final section, "Aspects of Islamic Culture," is set off by a really fine introductory essay by Albert Hourani. Major themes discussed in it are the continuing lines of both conservative and liberal change and reform in the eighteenth century; the one leading to a gradual opening to Western ideas, the other to such reformist movements as the Wahhabis. It is followed by an outstanding analysis by R. C. Repp of the rise of the Ottoman Learned Establishment hierarchy.

Hamid Algar then ably reviews ideological developments in Shi'i Iran during the century, focusing primarily upon a description of Nadir Shah's unsuccessful efforts to have Sunni governments ratify the proposed Ja'afari Shi'i school of legal

opinion, then moving on to describe briefly the emergence into public controversy of the Akhbari/Usuli debate within Iranian Shi'ism. The final two papers of the section deal especially with architecture. Aptullah Kuran offers a brief, well-illustrated survey of the form and facade of Ottoman monumental architecture from the sixteenth through the eighteenth centuries. John Carswell attempts to deal with the emergence of the decorative arts as an important focus of patronage in the eighteenth century. The paper relies on an odd mélange of sources and topics, which produce an only moderately successful blend.

In short, this collection is what one might expect of a colloquium of diverse scholarship held together by no more than the "Eighteenth Century." Anyone seeking a comfortable, clear-cut pattern to the century will not find it here. They will find, however, a useful vantage point from which to view the state of some portions of the field today, along with some interesting new material thrown in for good measure.

JON E. MANDAVILLE
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MESROB K. KRIKORIAN. *Armenians in the Service of the Ottoman Empire, 1860-1908*. Boston: Routledge and Kegan Paul. 1978. Pp. xii, 149. \$5.00.

The world knows the Armenians chiefly as subjects of the Turks and ultimately as their victims. The better informed may also be aware that the *millet* system allowed non-Muslims wide latitude in governing their own communities, while their role in the commerce of the empire has not been overlooked. It is also recognized that Greek and Armenian magnates were prominent in the capital, but the usual perception of Armenians in the eastern provinces is that of an industrious peasantry dominated either by Turks or by Kurds. Few outsiders are aware that eastern Armenians played substantial roles in the Ottoman state. That such was the case is amply documented by Mesrob K. Krikorian, prelate of the Armenian Church in Austria, who has painstakingly compiled the evidence in this book.

Basing his investigations on the provincial yearbooks (*salname*), which list the names, ranks, and functions of officials and representatives of local bodies, he has brought to light the services of numerous Armenians in the eastern *vilayets* (eight in the Turkish provinces and two in Syria). He has also assayed the difficult, perhaps impossible, task of estimating the population of Armenians and other ethnic groups in each of the ten *vilayets*.

His documentation suggests that, while the position of Armenians on representative bodies was

largely tokenism, they played a genuinely important, in some cases indispensable, role in the civil service. Krikorian finds them in telegraph offices, in provincial secretariats, and in the courts. He disposes of the canard that Armenians were prone to sharp financial practice; if that were so, it would be hard to account for the substantial number entrusted with budgetary and fiduciary responsibilities. They also figured frequently as *juges d'instruction*. Such delegation of authority attests to both a need and a willingness to rely on Christian subjects. Yet suspicion coexisted with trust. Turkish reserve is shown when the yearbooks are examined in an effort to find Armenians who served in the police forces. They were few and never in sensitive positions.

Naturally there were differences in jurisdictions. Krikorian finds that in *kazas* where there was European influence Armenian participation in government appeared greater than elsewhere. There is also a suggestion, which he does not develop, that Armenians became scarcer in the yearbooks, hence scarcer in professional employment, in areas near the Russian border. The presence of a highly aggressive Christian power at its back was surely a cause of unease to Turkish officialdom, especially because of consciousness of its own weakness. In the light of that weakness, the participation of the many Armenian officials Krikorian has been able to find is remarkable, as remarkable as his pertinacity in tracking them down.

JAMES B. GIDNEY
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HELMUT MEJCHER. *Imperial Quest for Oil: Iraq, 1910-1928*. (Monograph, number 6.) London: Ithaca Press, for The Middle East Centre, St. Antony's College, Oxford. 1976. Pp. 199. £7.50.

Helmut Mejcher's book seeks to examine the British government's involvement in oil concession negotiations in Iraq (formerly Mesopotamia) during the period 1910-28 and to view this involvement against broader British policy in the region. It is an expansion of the author's doctoral thesis on British mandatory rule in Iraq to 1926 and for this reason it is disappointing. The new sections on oil—about three-quarters of the book—do not have the scholarly depth or perspective one might otherwise have expected, and it gives the impression of being hastily assembled for publication. The book, unfortunately, contains too many sweeping assertions, imprecise statements, and some plain errors of fact and interpretation. It also has serious gaps, such as lack of investigation of the making of the Red-Line Agreement, the climax to the whole book.

Here are some short examples of the faults just listed. The statement that "Britain had to be alert lest any investment partnership was formed which might disturb the Middle East oil market" (p. 5), at anytime in the pre-war period, but particularly in its context of 1903-07, is nonsense. To say that "Eventually the [Shell] group was also to control the price of liquid fuel for the British Navy" (p. 6) is again, in the context, plain wrong, and should not have been made since the author has read, and relies heavily on, in places, the relevant article by M. Jack in *Past and Present*.

Most serious, perhaps, are the broad misinterpretations. Underlying it all is a basic misunderstanding of how Britain viewed Mesopotamia in this period. To conclude, as the author does (p. 168), that oil was the *ultima ratio* of all British policy, honestly admitted only by the Admiralty (the Foreign Office hid behind "smoke screens" of hypocrisy), is too simplistic. Oil considerations undoubtedly intruded into much of British policy making in Mesopotamia in this period. It would have been surprising if they had not done so, in view of Britain's vital strategic and naval needs and the secondary potential advantage of using oil revenues to pay for the mandatory administration. The author's conclusions indeed contradict his earlier statement (p. 31) that the Foreign Office "deemed it inexpedient that the A.P.O.C. [Anglo-Persian Oil Company] should become interested in Mesopotamian oil at all." This is just not true, as the author would have realized if he had made more use of the ample Foreign Office records on the subject or even used a 1968 doctoral thesis written precisely on this subject.

Other confusions derive from his uncritical acceptance at face value of assertions by individuals. A prime example of this is the importance he gives to Admiral Slade's memorandum of July 29, 1918 as representing official British government oil wisdom (pp. 35-39). The information that Slade's memorandum was challenged (it was, in fact, soon quietly dropped altogether) as representing an *ex parte* statement hostile to Shell, on behalf of the A.P.O.C., does not surface until later (p. 110).

The author does not, further, have any clear understanding of what the British government was about in its proposed take-over of Shell. He asserts, in consequence, that the San Remo Agreement "prevented Whitehall from taking steps to assume control of Shell" (p. 112), whereas in fact acquiring control of Shell was *dependent* on an Anglo-French Mesopotamian agreement.

This book shows up the dangers of "wild-catting" as a method of historical research. Random bores might produce some of the good oil but systematic drilling is the only way to show the true extent of the field and its significance. Had the

author followed this precept of closer attention to detail he might have avoided many of the errors and distortions his book otherwise unfortunately contains.

We can sympathize with him in one respect, however. Any author who has to suffer, as he does, the miniscule print, the unjustified right margin, the absence of foreign accents or italics in his printer's type, and the fact that on virtually every page at least one line of print is wildly crooked is hard done by. Unfortunately, so also is the reader.

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Y. PORATH. *The Palestinian Arab National Movement: From Riots to Rebellion*. Volume 2, 1929-1939. Totowa, N.J.: Frank Cass. 1978. Pp. xii, 414. \$27.50.

The Wailing Wall riots of 1929, Y. Porath believes, initiated new and major trends in both Palestinian Arab national politics and the development of pan-Arabism. His argument is convincing. The disturbances led to the involvement in the Palestinian struggle of the larger pan-Arab and Islamic movements and to a temporary victory in the Passfield White Paper. Violence thus appeared to be productive. But Zionist strength was also demonstrated immediately when the British retreated from the 1930 statement of policy. The result was increasing radicalization of the Palestinian Arabs and the decline of the moderates. The great increase in Jewish immigration and land purchases intensified this trend. Further British retreat before Zionist pressure (in the form of the withdrawal of the Legislative Council proposal in 1936) led to the national strike and the rebellion. At this stage Palestine and pan-Arabism were joined decisively. British proposal of partition led to the resumption of the rebellion, and British concern for British interests in the larger Arab world resulted in Arab victory in the form of the MacDonald White Paper.

The story has been told often, but never so well as Porath tells it. (The same is true of the first volume of this work.) His subject is the Palestinian Arabs, but British policies and actions receive the necessary attention. The documentation is admirable: Zionist and British official records, but most importantly Arab sources preserved in the Zionist and Israeli archives, Arabic memoirs, and the contemporary Arabic press. Porath has done far more than provide additional details and better documentation. He handles the documents with insight and has clarified many important episodes. In cases where the evidence is not as ample or direct as one would wish (e.g., certain activities of Amin al-Husayni), the exposition is persuasive.

Two topics are worth special mention. The treatment of land sales to Jews utilizes a great deal of new data and very fairly and perceptively assesses the impact of those sales on the Arab population and on Arab politics. Porath finds that the displacement of Arab peasants was greater than indicated by the French report, but still not massive. On the other hand, the new pattern of land purchases by Jews in the 1930s did contribute greatly to political radicalization. Of equal importance is the account and analysis of the Arab rebellion. The details—events, activities, names, and biographical data—are abundant and fully documented. The analysis is thorough, systematic, reasoned, and penetrating. In my opinion Porath seems to strive too much to find elements of class conflict and passes over indications that the rebel bands were organized and led by notables. Otherwise, his analysis of the course of the revolt and its failure is convincing. Such close examination of any aspect of modern Arab society is very rare. Porath's treatment is of prime importance to those who are concerned with the comparative study of society in general and of rebellion in particular.

This is in every respect an excellent piece of scholarship on a subject of major importance.

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EDWARD SPEARS. *Fulfillment of a Mission: The Spears Mission to Syria and Lebanon, 1941-1944*. Hamden, Conn.: Archon Books. 1977. Pp. xi, 311. \$19.00.

Sir Edward Spears died before completing this memoir, the last chapter being clumsily written by his second wife and secretary of fifty years, the editing superficially done by his grandson. The mission had been personally so excruciating that Lady Spears must have had quite a time getting the octogenarian Francophile-gone-sour to go through his diaries and papers relating to it. He had performed this liaison work with Charles de Gaulle and the Free French at the behest of his close friend and political ally, Winston Churchill, to whom Spears owed his influence since he had no Middle Eastern experience before World War II.

The first third of the book covers the problems Cairo faced in meeting the threat posed by Vichy collaborators with the Nazis in Syria and Lebanon before Allied operations succeeded there in June 1941. The second third describes how Vichy-Free French hatred and Anglo-French suspicions marred wartime administration, particularly in pledging Syrian and Lebanese independence and in feeding the people of these countries. The last third of the memoir focuses on the Lebanese crisis

at the end of 1942, when the French refused to recognize the recently elected government in Beirut and staged a coup, which led to considerable bloodshed before the British were able to restore the Lebanese government. If Spears had lived longer, he presumably would have covered the last two years of his mission in a similarly vivid, if all too predictable fashion.

That Spears did not is perhaps no great loss to history since his account is filled with bias, pique, and chauvinism. He takes revenge on Jean Helleu, Georges Catroux, and de Gaulle for the wounds they inflicted on his pride in the war or in their memoirs. Spears also singles out British personalities who got in his way: Generals Archibald Wavell and Henry "Jumbo" Wilson are depicted as narrow-minded commanders who were excessively cautious, while Anthony Eden, Harold Macmillan, and Alfred Duff Cooper are seen as too caught up with Foreign Office anxieties over upsetting the French. The only British leaders who appear sane are R. G. Casey, an Australian who served as minister of state in Cairo and supported Spears, and Churchill, of course. As for Lebanese and Syrian politicians, these are objects of Dickensian caricature. Passing figures from the U.S. and USSR are scarcely taken seriously.

The value of Spears's work is precisely its personal, political, and professional pettiness. For historians to dismiss this is to miss much of what made things happen as they did. Little mattered to Spears except winning the war and salvaging his honor, which he manages to do to his own satisfaction. For others, the whole mission may not seem to be so much one of "fulfilment" as one more example of British arrogance and amateurishness in the Middle East.

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HOSSEIN AMIRSADEGHI, editor, assisted by R. W. FERRIER. *Twentieth-Century Iran*. New York: Holmes and Meier. 1977. Pp. xv, 299. \$21.50.

For both the specialist and general reader, no Middle Eastern society is more problematic than Iran, save perhaps Israel. Iran is an excellent study in contradictions and contrariness, particularly for the historian. For instance, although it boasts the first successful constitutional revolution in the Middle East, the king continues daily to increase his autocratic and dictatorial hold over all phases of Iranian life. With an annual oil income approaching twenty billion dollars, placing Iran among the top producers in the world oil market, the per capita income of the majority of Iranian people remains below two hundred dollars, while unemployment and inflation persist.

Few accounts published in the last decade attempt to resolve these contradictions. Rather, the favorable accounts of Iran's political "stability" and economic "miracle" continue to swell the market shelves, with discussions of only the ruling Pahlavi family, the Iranian elites, and the petrochemical industries. They all fail collectively and individually to focus on the *society* under change over the last century, depriving both general reader and specialist of the opportunity to assess the Iranian balance sheet with greater accuracy.

Twentieth Century Iran, a collection of articles edited by Hossein Amirsadeghi, a writer, editor, and company manager, presents a much needed account of Iran from 1900 to the present. According to the editor, the "occasion was right" for the study of political, economic, strategic, and social factors that have thrust Iran onto the international scene bolstered by an "ambitious programme of industrialization and social reform." The work, divided into eight chapters written by a wide and diverse group of contributors, is meant to give the reader a "better appreciation of the forces working for change or continuity in Iran today."

Attractively bound and high priced, *Twentieth Century Iran* gives the reader more than the usual fare of high praise and short insight. Chapters one through four discuss the political formation of the present Pahlavi state from 1900 to 1976, with one chapter devoted to a rough and very disappointing listing of highlights of the oil industry. Chapters five and six enliven the rather deadening barrage of names and events of the preceding chapters with an enriched discussion of Iran's economy and society. The remaining two chapters, however, lower the reader once more to the dull tedium of political models, strategems, and futurism without a grasp of the flesh and blood of Iran, that is, the people and their historic aspirations.

In general, the collection is only partially successful in resolving the paradoxes found in Iran today. The presentation of several maps and the attempt to detail the "wide view" of Iranian history are admirable and rewarding. Interestingly, the photographs elucidate the purpose of the collection more than the editor's preface. The photographs of the Pahlavi kings visiting dams, oil refineries, and the late King Faisal are punctuated by early and recent pictures of Tehran, a gas-gathering point, Abadan Petrochemical Complex, and a tank of the imperial armed forces. The single photograph of villagers receiving land deeds does not offset the clearer intentions of the entire work. Despite the editor's claim, *Twentieth Century Iran* is another attempt to justify the presence of the kings, their retinues, armies, and oil technology. Except for Keith McLachlan's just analysis of Iran's shaky economy, misplaced priorities, and

lopsided investment patterns, and Michael Fischer's useful survey of available anthropological research into Iranian society, its people, and market systems, the collection is most disappointing in its shortcomings. For instance, no overall theme is stated, nor is any attempt made to identify the "forces working for change or continuity." Generally, the sources for the historical articles rely on secondary materials with passing use of state papers or Persian accounts. While limiting the narrative to the rise of the Pahlavis, the historical chapters omit well-known data, such as the Mahabad Republic of 1946, the CIA role in the 1953 coup of the Mossadeq democratic government, the role of CIA and military advisers in creating the Security Police (SAVAK) and antiguerrilla unit (Rangers). Finally, dishonest and unprincipled treatment of well-known events characterizes this work: the distortions of the religious leaders' positions such as that of Ayatollah Khomeini's protests in 1963; the characterization of the writers as alienated; and the failure overall to deal fairly with the alternatives to political action and rule over the last seventy years, such as the Gilan Autonomous Republic, 1920-22, or the rise of armed struggle after 1971 beginning with the Siyahkal uprising.

The contradictions and contrariness of Iran's history are thus only partially resolved by *Twentieth Century Iran*. General readers and specialists will be able to glean some excellent insights into Iran from those parts dealing more frankly and concisely with the people and society. In reading the present work, however, the need to focus on the history of the society, the Iranian people, and their struggles will be clearly demonstrated. Amirsadeghi has indeed assisted us partially along that still untrod way.

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AFRICA

HENRY S. WILSON. *The Imperial Experience in Sub-Saharan Africa since 1870*. (Europe and the World in the Age of Expansion, number 8.) Minneapolis: University of Minnesota Press. 1977. Pp. xv, 415. \$18.50.

This book is the eighth volume in a series entitled *Europe and the World in the Age of Expansion*, edited by Boyd C. Shafer. In this sweeping survey the author describes the European impact upon Africa from 1870 to the present. In most such surveys there are seldom any startling new theories of the European experience in Africa, and this work is no exception. The organization of the chapters follows a logical pattern commonplace to the histo-

rians of Africa who have written on this period and would serve as an admirable text for a course designed to teach Europe's involvement in the African continent. The account moves chronologically forward from the partition of Africa to the aftermath of empire. The subjects—"The Consolidation of Colonial Rule," "The White Man's Realm," "The First World War," and so forth—unfold clearly with selected events to buttress the descriptions.

Few can quarrel with the obvious, but in the illustrations to support the major themes the examples demonstrate the author's predilection to select those derived from his own scholarly preferences. This is, of course, to be expected, but it results in certain shortcomings by over-emphasizing some areas of Africa while ignoring others. Thus, the book is dominated by events in West Africa in comparison to activities elsewhere. Certainly less is written of East and Central Africa, and nothing about the Sudan, Ethiopia, or the Horn of Africa. This imbalance pervades most chapters and is not remedied by a cursory chapter on the Germans and the British in East Africa.

The emphasis on Western Africa occurs from the very beginning in the chapters dealing with the Scramble and results in a significant and perplexing omission. If one interprets the partition of Africa from a West African perspective, the Scramble is indeed best explained by economic causes. Such reasons cannot, however, satisfactorily explain the partition in other regions of the continent. In a survey of this kind to ignore the role of Egypt, the Nile Valley, and "the official mind" in the partition of Africa is most surprising. Although included in the bibliography, no discussion is made of the seminal work of Ronald Robinson and John Gallagher, *Africa and the Victorians*, published nearly fifteen years ago. Moreover, to blandly accept the proposition that France initiated the Scramble must not go unchallenged. The Germans, King Leopold, and the British themselves are all prime candidates for that dubious distinction. Surely even the most entrenched economic determinist would have to place the partition of Africa into a wider perspective where strategic considerations, philanthropic interests, and the actions and personal ambitions of individuals, with little concern for economic consequences, drew Europe into Africa.

There is an excellent bibliography which undoubtedly attests to the wide reading of the author, but curiously Basil Davidson, who has done so much to elucidate and champion the African past, is not included.

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Santa Barbara*

ANDRZEJ DZIUBIŃSKI. *Miedzy mieczem głodem i dżumą: Maroko w latach 1727-1830* [Amid Sword, Famine, and Plague: Morocco in the Years 1727-1830]. Wrocław: Ossolineum. 1977. Pp. 267. 65 Zł.

Andrzej Dziubiński, an author of several monographs on the history of Morocco, deals in this book with the period which, in his view, generated the deeper causes of that country's eventual loss of independence by the early twentieth century. It was an age of multifaceted crisis from which no vigorous or sustained recovery was possible. This time of trouble stretched over a century, from the death of a strong-handed sultan, Mulay Ismail, in 1727, until the conquest of neighboring Algeria in 1830 by France, the power that henceforth was to dominate the northwest of Africa.

With the end of what the author terms the "pax Ismailiana," Morocco entered three decades of civil wars arising from the unsettled law of succession in the Alawid dynasty. In reality, the focal issue of the upheavals turned out to be the emergence of centrifugal forces, represented by the religious leaders, the *marabouts*, the burghers of Fez and other towns, and, above all, the Berber tribes. Their antagonism was directed not so much against the dynasty itself but rather aimed at its military mainstay, the black pretorians called 'Abid al-Bukhari, who on their part were wont to make and unmake the Alawid sultans. The 'Abids' dominant position in the power structure was challenged and by the end of the civil wars was reduced to the level of that of rival groups. The reign of Sidi Muhammad (1757-90) brought a halt to the rampant anarchy and with some degree of success this able ruler attempted to reverse the erosion of central authority. Yet he had to contend with a wider array of potential opposition. Unlike some other writers, Dziubiński stops short of over-rating Sidi Muhammad's long-range historical significance, and points out that his successors inherited a shaky throne.

The account of political turmoil takes up a good third of the book, and the author does not escape the temptation to present it as a catalog of coups, palace revolutions, tribal rebellions, penal expeditions, and mass executions. Fortunately, in the remaining parts he tackles the more ambitious task of analyzing the interrelation between the crisis of the state and the condition of the society, economy, and armed forces. He emphasizes the fact that in the midst of political instability natural disasters acquired catastrophic proportions. While famines, droughts, and bubonic plague ravaged the country, the government did little more than collect ever increasing taxes. The army, whose main function became to replenish the treasury by enforcing taxation, at the same time was the heavi-

est of the state's financial burdens, especially as the military rebellions usually ended in generous increments of pay for the troops. Meanwhile their fighting ability remained low and the attempts to introduce modern weapons or organization without resorting to nonmilitary reforms proved of limited effectiveness. On the eve of the French invasion of Algeria, Morocco possessed little defensive strength.

The chapters dealing with the army, economy, and, particularly, the impact of natural disasters are impressively documented with data drawn from Western, largely French records. Although the author complains about the relative shortage of sources in Arabic, their total absence in the bibliography appears a weak point of the book. Even the works of two chroniclers of the period, Nasiri and Zayyani, are quoted from French translations. Still, Dziubiński is fond of using Arabic terms or expressions (and at that without italics), a proclivity that impairs the readability of the text, the more so that the transliteration system is not the one generally accepted in world literature but phonetically Polish.

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JOSEPH P. SMALDONE. *Warfare in the Sokoto Caliphate: Historical and Sociological Perspectives*. (African Studies Series, number 19.) New York: Cambridge University Press. 1977. Pp. xi, 228. \$24.95.

Most precolonial African societies have experienced the ramifications of warfare. It is axiomatic that this multifunctional institution affects the entire sociocultural and political order. And yet a societal analysis from the perspective of warfare has been neglected by Africanists.

Joseph P. Smaldone examines the historical development and sociological implications of warfare in the emirates of the Sokoto Caliphate in nineteenth-century northern Nigeria. He analyzes the growth of militarism and the degree to which it was displayed by focusing on the revolutions in military technology and army organization and their influence upon state structure. He discerns three evolutionary phases. In the formative *jihad* period (ca. 1790–1817), the military force was a “raiding citizen army.” All Muslim males fought to defend Islamic ideals under charismatic leadership committed to the maintenance of an egalitarian polity with undifferentiated institutions. This changed during the era of consolidation from 1817 to 1860 as a palace army evolved, primarily a result of the increased use of the war horse. Although Smaldone does not attribute feudalism to

the mid-century Caliphate, characteristics of the “feudal state” are applicable. The horse fostered the development of three feudal institutions: vassalage, fief holding, and the cavalry. Since the horse was the base of the cavalry, and only fief holders had the resources to purchase and maintain horses, fief holding became the central institution of the politico-military conformation. Control over the distribution of horses gave responsibility for military levies to the aristocracy and thus encouraged the emergence of a new warrior elite. Accordingly, the existing feudal relationships precipitated a distinction between cavalry and infantry forces, spurred a devolution of power, and enhanced aristocratic control over politico-military functions. The introduction of large quantities of firearms after 1860 instigated the transformation from a feudal to a bureaucratic and centralized form of organization. Firearms made possible an infantry-based standing army and a corps of slave musketeers, who were dependent upon the state for support. And control over the supply, distribution, and use of modern weaponry stimulated an unprecedented concentration of power and authority in the hands of the Fulani emirs. The politico-military institutions became increasingly bureaucratized as key positions in the government and military were given to unlanded freemen and slaves. By relieving the fief-holding aristocracy from its responsibility of military command, the Caliph removed aristocratic control over the military levies and transferred the socioeconomic base of power from landed estates to the state.

Smaldone effectively employs a diachronic or process model to illustrate his thesis that political structure is dependent upon military organization and that socioeconomic structures are influenced by politico-military institutions. This is one of his most successful adaptations of sociological theory. Other attempts are not as convincing. For instance, he uses systems theory, although never clearly defined, to demonstrate how functional relationships interact and change. Within this approach Smaldone attributes sociopolitical change to technological innovation and warfare. This precludes the possibility of change coming from another source because the other social units (or institutions) are not seen as having equal input into the system. Could structural change also have been caused by economic, religious, and political factors independent of the military complex? Indeed, Smaldone's ambitious, well-argued conceptual framework asks the reader to consider whether Sudanic military traditions can be placed at the core of communal sociopolitical values, or was this militarism an integral aspect of a more pervasive national identification? Consideration of this problem may have been possible if Smaldone

had relied less heavily on nineteenth-century European source materials and more on Arabic documentation. Its importance outweighs the attention devoted to military detail, combat conditions, and the role of the soldier.

This study is especially valuable for scholars who have an interest in comparative history. Smaldone's extensive bibliography and references reflect his multidisciplinary orientation. The work complements the scholarship on precolonial Hausa-Fulani society, and its contribution enriches the field.

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JOSEPHINE F. MILBURN. *British Business and Ghanaian Independence*. Hanover, N.H.: University Press of New England, for the University of Rhode Island. 1977. Pp. ix, 156. \$12.50.

Ghana's prospects for development were widely acclaimed when it became the first sub-Saharan nation to attain political independence after World War II. Yet today its economy is characterized by stagnation: high unemployment, a swollen international debt, skyrocketing inflation, and the continuing enrichment of a narrow class of business managers and top government personnel in the midst of mass impoverishment of the peasantry and working people. One might, therefore, look forward with anticipation to reading Josephine F. Milburn's book, *British Business and Ghanaian Independence*, in hopes of discovering at least some elements of an explanation for this sad outcome.

Milburn declares at the outset that she seeks to assess the relations between business firms and the colonial and postcolonial governments in order to evaluate the "generally popular views about business" ranging from those of the British administrators to those, which she attributes to Lenin and Nkrumah, advocating the "conspiratorial nature of business." Her evidence, much of it from government and company documents, relates primarily to the marketing activities of the three big cocoa firms, the United Africa Company, John Holt, and Cadbury, during the 1937-57 period. She gives only a superficial summary of the post-independence relations of business and government. She concludes that "foreign investment in cocoa has contributed to development of a central monetary market in West Africa and has brought about the conditions and motivation necessary for economic and national development" (p. 116).

The scope of her analysis, as well as her conclusion, is, however, seriously limited by her narrow analytical perspective. She describes in great

detail the sometimes cooperating, sometimes conflicting, efforts of the colonial firms and administrators to establish viable, continuing cocoa marketing structures, especially during the turbulent times of the cocoa-holdup and the postwar independence struggles. But she obscures the reality that, whatever their disagreement over details, the colonial firms and administrators did work together over the years to forge an institutional framework which shackled the Ghanaian peasantry and economy to production of ever increasing amounts of cocoa beans for the firms' profitable manufacture and sale in the context of their global operations.

Milburn's book does not really explore the wealth of evidence about the way this initiated the "development of underdevelopment" process which lies at the root of Ghana's current predicament. The issue is not, as she suggests, whether the companies conspired with the government. Rather, it is that the inherited institutional structure, shaped under the protection of the colonial umbrella, enabled a handful of oligopolistic firms to accumulate and reinvest capital in the extraction of raw materials and sale of manufactured goods in the particular conditions of the then Gold Coast. The mass of the African peasantry was alternately enticed and coerced by the imposition of the money economy to provide the essential cheap labor. Tens of thousands worked as sharecroppers, investing their time and muscle in planting and harvesting cocoa from which the companies, through their control of the marketing process, reaped the profits. Only a limited class—those who accumulated capital by hiring labor and investing in speculative trade and real estate or who became managers and supervisors in the companies or government posts—participated in the benefits of the resulting distorted development. These, too, were the main beneficiaries of the Africanization programs which Milburn praises as company contributions.

In short, the detailed government and company documents which Milburn quotes at length may interest those concerned with minutiae of company-colonial relations. They do not appear to support her unduly optimistic conclusions. Instead, her analysis tends to obscure the underlying institutionalization process that lies at the root of the problems that plague Ghana to this day.

ANN SEIDMAN
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RICHARD A. JOSEPH. *Radical Nationalism in Cameroun: Social Origins of the U.P.C. Rebellion*. (Oxford Studies in African Affairs.) Oxford: Clarendon Press. 1977. Pp. x, 383. \$34.50.

Richard A. Joseph is an Oxford graduate from the West Indies who teaches political science at the University of Khartoum. His research on the political evolution of the French Cameroons forms part of a second generation of investigations made possible by the opening of the interwar archives in Paris and the records of the Direction des Affaires Politiques et Administratives (APA) at Yaoundé through 1949. On the basis of these materials Joseph previously published articles on Douala Manga Bell in Paris (1919-22), the German question (1919-39), the riots of September 1945 at Douala, and the origins of the Union des Populations du Cameroun (UPC) in April 1948, upon which he draws in this study. But Joseph did not employ the annual and quarterly reports of the French heads of regions and subdivisions from the early 1920s on, which contain detailed economic information as well as important data on the UPC's local activities. (These reports were available through 1954 to the reviewer and other researchers at Yaoundé in the early 1960s.)

Thus Joseph relies almost entirely on published materials and interviews for his economic analyses. At the same time, while Joseph interviewed many French and Cameroonian participants and observers of the events of decolonization, he did not consult the American Presbyterian missionaries who knew Ruben Um Nyobé and other Bassa and Bulu Upécistes well. In particular, they could have helped him to make a more accurate evaluation of Um Nyobé's personality and abilities. These deficiencies in sources make Joseph's work much less complete and authoritative than it might otherwise have been. Further, the unavailability of the APA records after 1949 or any of the Paris archives for the postwar period leaves much of his study subject to revision or refinement within another decade or so.

Joseph's contribution to our understanding of decolonization in the French Cameroons is nevertheless important. His study sheds much light on the socioeconomic bases of the political behavior of the main groups supporting the UPC (the sub-proletariat of New Bell; the Bassa peasants of the Sanaga-Maritime; the Bamiléké farmers, traders, and transporters; and the civil servants whose advancement was blocked) and to their radical actions or their defections under pressure and repression by the French administration. It analyzes effectively the behavior of the Beti and Bulu cocoa producers and the peoples of the north among whom the UPC failed to win much of a following. The work shows the important role of the French settlers in influencing the course of events, an aspect heretofore largely neglected except in connection with the 1945 riots. It provides new data on Cameroonian political activities in the years from

1938 to 1945 and a much more lucid and detailed account of the political events between the May 1955 riots and the December 1956 rebellion than earlier works. Finally, Joseph's study illustrates the contribution that an intelligent use of the methodology of political sociology can make to understanding political behavior in an African setting.

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B. A. OGOT, editor. *Kenya before 1900: Eight Regional Studies*. Nairobi: East African Publishing House. 1976. Pp. xix, 291. 55.00 sh.

Kenya before 1900, edited by B. A. Ogot, is a welcome contribution to East African historiography. It consists of eight regional studies in which each author reconstructs the precolonial past by critically using oral traditions, written sources, and the techniques and methodologies of other fields. The result is a genuinely interdisciplinary synthesis. Solid, professional microstudies such as these will enable historians to one day draw the whole, broad picture of Kenyan history. The articles analyze the variety of political, economic, and social changes that took place in these societies before the advent of the West. Their recurrent theme is the dynamism and innovativeness of each group in coming to terms with its surroundings. Most of these articles synthesize larger works on these topics by the same authors, and they whet the appetite of the reader to delve more deeply into area studies.

C. Ehret's excellent analysis of social and economic change in western Kenya gives a broad understanding of the culture and behavior of the ordinary people. J. Sutton's archeological skills coupled with his ability to present complex material in a clear, straightforward manner, make his article on the Kalenjin first-rate. The treatment of the Sirikwa is particularly impressive. R. Blackburn provides a useful study of Okiek hunters and gatherers and dispels several misconceptions about them. One only wishes he could have analyzed in more depth the sources of those misunderstandings. W. Ochieng's fine article on the Gusii before 1900 illustrates how a small group of people were able to avoid being assimilated by their larger neighbors and how they became the "granary" of the Luo. G. Muriuki's chapter on the Kikuyu in the precolonial period aptly analyzes the question of land tenure and the *mariika* system. One regrets that space did not permit him to delve in more detail into the relationship of the Kikuyu and the Maasai. J. Fadiman on the Meru people and T. Spear on the Mijikenda both do superb analyses of

oral tradition. K. Jackson's fine work on the Kamba includes a section on the peopling of the Kilimanjaro area and offers an interesting explanation for the establishment of the many communities in the region. The reviewer's own research in Taveta corroborates his conclusions.

Each article is well annotated and there are sufficient maps. This is a book which belongs on every Africanist's bookshelf.

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DAVID WILLIAM COHEN. *Womunafu's Bunafu: A Study of Authority in a Nineteenth-Century African Community*. Princeton: Princeton University Press. 1977. Pp. x, 216. \$15.00.

This is a short book on a narrow subject, but one that should command the attention of anyone interested in the history of Africa or of nonliterate societies anywhere. Through a clear and cogently written narrative, David William Cohen exposes to our view a wide range of African political traditions within a small, even marginal African community. The resulting vision of ground-level local political authority has the possibility of altering our understanding of the nature of politics and community in precolonial Africa, and even the history of village societies elsewhere.

Womunafu (ca. 1820-1906) was born both a prince of the royal house of a petty state in eastern Uganda and under the sign of possession by Mukama, a departed culture hero and diety. This alone would not normally attract the attention of historians, as his religious powers were not unique and his political influence circumscribed. He is by these measures of his importance to events and large numbers of people an unlikely subject of history or biography. His life was lived within a narrow world of geographic and political boundaries, in a well-deserved obscurity beyond the limits imposed by small-scale society. In Cohen's deft and penetrating analysis, however, his life comes to illuminate the complex and contradictory nature of political authority in village society. The transformation of the young Mukama, whose prestige derives initially from the spiritual authority of possession, into a secular prince relying on the mundane authority of family, land, and following (standards of worldly power among the Soga people), is the central drama and narrative core of this book. By following this transformation as Womunafu passes from one life stage to the next, from one homestead to another between his birth and his death in 1906, we come to understand the depth and nature of his dual authority and its social and cultural underpinnings as they were regarded by his African contemporaries.

Similarly, the village of Bunafu is neither typical nor central to the history of its region and tells us little directly of the history of Africa or Uganda or even Busoga. It does, however, provide the arena in which the emergence of Womunafu as its dominant figure becomes an avenue for understanding the dynamic nature of political authority, allowing us to transcend a static vision of village life inherited from social anthropology. Here the small daily dramas of individual ambition, marriage and kinship, migration and resettlement are played out in their kaleidoscopic complexity. Here the reconstruction of these mini-events into a pattern of political authority brings us far closer to the realities of life in an African community than a dozen histories of kings and kingdoms could possibly take us.

In addition to a graceful style and penetrating analysis of the narrative traditions of an African community, Cohen has also once again advanced the methodology of African and oral history. In an earlier book (*The Historical Traditions of Busoga* [1972]), he examined traditions of migration on a large scale and helped reconstruct central themes of early Uganda history. Now, he again mines the raw material of traditions of migration, tapping the smaller veins, the local traditions of the origins and movement of clans and families in a single village or district, with remarkable success. Not only are the traditions themselves examined for information conveyed in conscious narrative form, but the collective data are mined for information on marriage patterns, births, and deaths which, like the social history of literate societies, begins to give us a view of history as it may have escaped those living it. His appendix on chronology similarly advances our handling of that difficult subject as it relates to nonliterate societies.

My only objection to this work of intense scholarship and sharp intelligence is that the infelicitous and, for some, unpronounceable title, may keep many nonspecialists who might learn from it from opening its pages. The greater the pity, as the rewards of mastering the difficulties of *Womunafu's Bunafu* far outweigh the cost of concentration and purchase price.

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L. H. GANN and PETER DUIGNAN. *The Rulers of German Africa, 1884-1914*. Stanford: Stanford University Press. 1977. Pp. xiii, 286. \$17.50.

This book is the first in a multivolume project which will describe and analyze the colonial services of the various European powers in their Afri-

can possessions. The complete project is bound to make an extremely valuable contribution to the history of Europe in Africa and, as such, it is a further confirmation of the shift in scholarly interest away from the politics of the "Scramble" to the study of colonial structures and techniques and of the sociology of the administrators.

If this first volume, which shows how the Germans ran their empire in Africa, is any guide, then the whole series is assured of success. Beginning with an analysis of the colonial enthusiasts in Germany itself, and with the imperial government's slow and halting attempts to develop a machinery of central administration, the book goes on to describe the way in which forms of control evolved for regions as different as Togoland and Southwest Africa. The central chapters then provide a detailed sociological analysis of the civilian administrative service and—of almost equal importance, given their numerical proportion—of the military men who were also stationed in the colonies. The economic development of Germany's African colonies gets almost an equal amount of space, and the book concludes with a consideration of the social impact of this conquest and rule, together with a description of the fall of the German colonial empire during the First World War.

Without doubt, this is the best single-volume study of the German colonial service in Africa. It brings together a host of useful details upon the social background of the rulers, the characters of the various governors, the nature of the bureaucratic machinery they operated, the failures and successes of economic exploitation, the civil-military (and, even more important, civil government-settler) relations, and the impact which the possession of an extensive colonial empire had upon both German society and its public consciousness. This evidence is supported by dozens of statistical tables, and the style throughout is impressively clear—augmented by a very fine deployment of anecdotes, character sketches, and geographical descriptions.

If the present reviewer had certain reservations about the book, they are of a minor order: for example, that the authors really should have allocated more space to the social impact of conquest to deal adequately with such a large-scale theme; that they probably underemphasize the importance which the possession of colonies had in Germany, if not to the people as a whole, then certainly to some influential groups; and that they seem unduly apologetic about the darker aspects of German colonial rule. All in all, however, *The Rulers of German Africa* will be a most useful acquisition to any scholarly library.

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ASIA AND THE EAST

DANIEL H. BAYS. *China Enters the Twentieth Century: Chang Chih-tung and the Issues of a New Age, 1895–1909*. (Michigan Studies on China.) Ann Arbor: University of Michigan Press, for Center for Chinese Studies. 1978. Pp. xi, 295. \$17.50.

During his 1967 sojourn in Tokyo, this reviewer held a long talk with Chang Yen-ch'ing, Viceroy Chang Chih-tung's son. He was uncertain as to whether the viceroy would receive a fair evaluation by Western scholars. Daniel H. Bays's book—along with other Western works on Chang Chih-tung—assures us that Chang Chih-tung's contributions to the late Ch'ing modernization drive will, indeed, receive a fair but critical reassessment.

In five out of the nine chapters, the author deals with the role of Chang Chih-tung based on two themes: reformism and nationalism. Chang emerges as a "civilized scholar-statesman" as well as a moderate elite-reformer with a nationalistic commitment. Bays argues that earlier theories of regionalism in late Ch'ing modernization will not be adequate to provide a true interpretation of the role of the elite reformers, who still operated in concert with the policy of the central government. One has little difficulty in accepting Bays's conceptual themes of reformism and nationalism, but it is not easy for some to repudiate regionalism altogether. In fact, Chang Chih-tung himself exemplified the best efforts of regional modernization despite the court officials' efforts to undermine his endeavors.

The volume has excellent source information and also presents valid analyses. However, some reservations about the author's selection of both materials and issues merit further discussion. Bays discusses with competence the issues concerning the viceroy's involvement with railway construction, the constitutional movement, the reform of 1898, the Boxers, the student revolutionary movement, and provincial and national politics. But he left out one important economic issue, Chang Chih-tung's strenuous but successful efforts to defeat the U.S. plan to convert China's silver standard to a gold standard. This was proposed by Jeremiah Jenks, who then headed the U.S. congressional delegation that advised the Chinese government on monetary reforms, upon the latter's request.

Economic crises of the late Ch'ing were largely due to the government's gross mismanagement of national resources as well as to its military expenditures in both foreign and domestic wars. The war indemnities resulting from the Sino-Japanese War of 1895 and the Boxer War of 1900 had put

China into a financial strait-jacket of foreign debts and trade deficits. As of 1903, China and Mexico were the only two nations with silver standard monetary systems. As the price of silver plummeted on the world market, the foreign indemnity powers began to press China to convert its silver standard to a gold standard in order to protect the value of their indemnity receipts.

At the same time, Robert Hart proposed a tax reform based on a land tax. Taking Jenks's monetary reform plan with Hart's tax proposal, Chang Chih-tung sensed foreign collusion directed toward economic colonization of China. Therefore he waged a bitter fight against both plans on the grounds of economic nationalism, trade protectionism, monetary independence, and fiscal sovereignty. Bays's argument on nationalism and reformism could have been much stronger if he had not omitted this important economic issue.

In order to understand Chang's nationalism, one must appreciate Chang's commitment to modernization. His message was loud and clear: China must progress to survive. Whatever could be transplanted and grafted onto the Chinese trunk must have "Chinese roots" and must be coterminous with the national interests of China and the welfare of the Chinese people. The legacy of Chang's *chung-t'i hsi-yung* (Western knowledge to serve Chinese principles) still remains a formidable challenge to today's debate on "whither China's modernization?"

As for the bibliography, Bays omits many source materials. In analyzing Chang's administration in Hupeh, he neither lists nor consults Chang Chih-tung's *Chang-wen-hsiang-kung chih-e-chi* (Studies on Chang Chih-tung's Administration in Hupeh); in discussing Viceroy Chang's leadership and his relations with his staff members, Bays fails to mention the indispensable *Recollections on Chang Chih-tung's Secretariat Staff* and *The Story of A Chinese Oxford Movement* by Ku Hung-ming. In addition, he leaves out Shen Yu-ch'ing's *Tao-yuan-chi* (Shen Yu-ch'ing Miscellany), as well as Wang K'ang-nien's *Wang-hsiang-ch'ing hsien-shen pi-chi* (Wang Hsiang-ch'ing Miscellany). Bays also fails to mention William Mesny, although Mesny, who served as Chang's foreign advisor, was instrumental in shaping Chang Chih-tung's modernization program in subsequent years.

Despite these limitations Bays's book will no doubt contribute considerably to a reassessment of Chang Chih-tung's role in China's modernization drive during the late Ch'ing.

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LI CHI. *Anyang*. Seattle: University of Washington Press. 1977. Pp. xviii, 304. \$25.00.

In 1928, Li Chi, a young Chinese scholar with the unique distinction of a Western degree in anthropology, was chosen to head China's newly formed archeological research organization and to direct excavations near the town of Anyang. The site could not have been better selected. Digging revealed the last capital city and royal tombs of the Shang dynasty and proved the historicity both of oracle bone inscriptions and of the dynasty that produced them. Work at the site came to an end with the outbreak of war in 1937, but Li carried on, supervising the analysis of the finds and the publication of an extensive series of reports about them in Chinese. Now he has given us a general account in English of the work to which he and his colleagues have devoted the last fifty years.

In the last chapters of his book, Li summarizes what his group has learned from its studies about such characteristics of late Shang times as architecture, economy, decorative arts, and worship. These pages are concise and clear. Unfortunately, Li's concentration on Anyang and on the research of his own team has led him to neglect material from other Shang sites, which would enhance understanding of the Anyang finds, and to omit interpretations made by scholars not directly associated with the Anyang project. The book's bibliography reveals the same omissions.

The heart of the book, however, lies in its earlier chapters, devoted to the development of Shang studies and archeology in China, to a year by year account of the Anyang excavations, and to the turmoil in scholars' lives caused by the war and its aftermath. Here are sharply etched biographies of the great men in Chinese archeology with whom Li was intimate during his long career. Here we see the challenges faced by the pioneer in archeology—the need to breach the barriers of traditional scholarship, disarm political hostility, end the depredations of the curio hunters, and unravel the mysteries of previously unknown signs in the earth. Paradoxically for a book dedicated to the distant past, *Anyang's* greatest value is found in its history of a major branch of modern Chinese scholarship.

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PAUL J. HINIKER. *Revolutionary Ideology and Chinese Reality: Dissonance under Mao*. Introduction by ITHIEL DE SOLA POOL. (Sage Library of Social Research, number 47.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. 320. Cloth \$14.00, paper \$6.95.

This volume purports to "explain the motivations underlying the Great Proletarian Cultural Revolu-

tion in China" by using what the author calls "derivations from an established social science theory, that of cognitive dissonance." Paul J. Hinder, associate member of the Center for Far Eastern Studies at the University of Chicago, begins with the assumption prevalent in the 1950s and 1960s that the Great Leap Forward was a colossal failure and an aberration. He proceeds to "prove" that, confronted with "disconfirmation" of ideological promises of an imminent Communist utopia, Mao, the "besmirched" prophet, and his committed followers intensified their proselytizing to seek new support for the "shaken ideology" from certain strata of the Chinese population, resulting in the polarization of the party leadership and the Cultural Revolution.

However, aside from the author's questionable assumptions, the thesis is not supported by the massive "evidence," which consists of lengthy quotations from speeches, resolutions, and survey data that are not specified but apparently refer to the author's earlier interviews of mainland refugees in Hong Kong. In twelve disparate chapters, three appendixes, and copious footnotes, the author traces the major doctrinal developments in China from the Lushan conference in 1959 to the end of the Cultural Revolution, and in the process tries to show the widening cleavage between Mao and his opponents, with the former increasingly distressed by cognitive dissonance and distorted perceptions. Based primarily on secondary works and translations from the Chinese, the narrative is often repetitive and sometimes irrelevant and contradictory, with occasional factual errors (Hinder twice identifies Chang Wen-t'ien as a military professional, he assigns the establishment of the Chinese Soviet Republic to December 1931, and he consistently misspells Dairen as Darien). But the most serious flaw is in the author's attempts to relate the events he describes to his thesis. For example, to use the number of articles attributed to certain members of the Politburo or the number of treaties signed with Third World countries after 1959 as indicators of increased proselytizing for Mao's ideology is at best arbitrary.

The Great Leap Forward and the Cultural Revolution are epochal events in the history of the Chinese Revolution, symbolizing certain stages of its development, which cannot be understood *in vacuo*. Far from being spawned in someone's millennial fantasy, the Great Leap Forward marks the completion of the transformation of the base of Chinese society, while the Cultural Revolution completes the revolution in the superstructure. The establishment of communes in 1958-59 may have caused some confusion in the economy, but since the Cultural Revolution, what was called a utopian dream has been institutionalized and ac-

cepted by Mao's supporters and critics alike. This raises some fundamental questions about the author's theory.

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JOYCE LEBRA *et al.*, editors. *Women in Changing Japan*. Stanford: Stanford University Press. 1978. Pp. xi, 322. Paper \$5.95.

Women in Changing Japan was undertaken by a group of "American women engaged in studying various aspects of the life patterns of Japanese women in many walks of life" in order to "ascertain the degree to which women in Japan today are departing from the traditional feminine ideal—an ideal that prescribed a domestic, subordinate, and often subservient role for women" (p. iv). Among the authors are three university professors, three graduate students, and six other women, most of them current or past residents of Japan.

The book contains an introductory essay on the "Evolution of the Feminine Ideal," eleven chapters on women in different work settings (farming, factories, offices, family businesses, service industries, bars, teaching, the professions, the media, politics, and sports), a chapter on "Women and Suicide," and a summarizing chapter by the senior editor. As a general rule, each essay contains a historical sketch of women in the occupation under consideration, an overview of the contemporary scene, and a vignette or two in the form of personal histories. Materials for the essays were gathered from interviews and surveys conducted in the early 1970s. Sources are cited in footnotes and there is a lengthy bibliography, which readers should find helpful.

The essays are of remarkably uneven quality. A majority of the chapters are badly conceived, poorly organized, repetitious, and superficial. Some contain factual and spelling errors. None offer full explanations of the sampling procedures the writers employed. Of those that do mention sample size, in all but three cases the samples are too small to permit confident generalization. The moribund concept of a "traditional ideal" is a befuddling mist clouding the entire undertaking. A number of essays, almost inadvertently, illustrate that the women's "ideal" in Japan has changed markedly among classes and over time. But virtually no one addresses this problem directly and tries to relate empirically the diversity of the past to the expanding diversity of the present. Most puzzling of all is the complete absence of a chapter treating the changing roles of women in the setting where they are most numerous and significant, as wives and mothers in the home.

The book is partly redeemed by its bibliography and a handful of thoughtful, well-researched chapters that include an informative account of women in service industries, an illuminating essay on female teachers, a sensitive analysis of farm women, and an excellent ethnography of Ginza bar hostesses.

GARY D. ALLINSON
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DAVID LELYVELD. *Aligarh's First Generation: Muslim Solidarity in British India*. Princeton: Princeton University Press. 1978. Pp. xxii, 380. \$25.00.

Once in a while there appears a book which, while seemingly defined by a rather narrow subject matter, and perhaps even the outgrowth of a dissertation topic, illuminates to an unusual extent a range of far broader intellectual concerns. In the better-trodden paths of historical research such an effect is now hard to achieve, but in the history of the Indian subcontinent, where so much original research is waiting to be done, the occurrence is much less rare. Yet having said that, it is exceptional to encounter a monograph as original and thought provoking as David Lelyveld's account of the founding and early years of the Muhammadan Anglo-Oriental College at Aligarh, once the *bête noir* of the Indian National Congress, and today Aligarh Muslim University.

Here, then, is a book, judiciously argued and written in a style that carries its learning lightly, which throws new light on the nature of Muslim elite culture in northern India during the nineteenth century, on the growth of a Muslim communal identity, and also on the history of Indian education. Only in the first chapter is Lelyveld tempted to lapse into jargon, as when he states that the Mughal concept of society "vested authority in a network of kin-like units bound in a system of asymmetrical exchanges that reflected their hierarchical relationships" (pp. 20-21). Otherwise, this is a richly-endowed, rewarding piece of scholarship in which, at one point (pp. 258-59), the author even attempts to explain how cricket is played!

Lelyveld begins by defining the self-perceptions of the Indian Muslim community during the nineteenth century, explores (in probably the most absorbing section) the ambiguous place of *sharīf* culture within the framework of the British Raj, and then considers the influences that shaped Sayyid Ahmad Khan's dream of drawing the sons of the Muslim elite into the mainstream of "Western" education, leading to the founding of the Muhammadan Anglo-Oriental College at Aligarh in 1875. In the last three chapters Lelyveld exam-

ines closely the impact of the Aligarh experience—the curriculum, the intellectual ideals of its administrators and teachers, the *esprit de corps* of boarding-school life on students introduced to it from the insulated security of traditional Indo-Islamic family life, and finally, the way in which the Aligarh experience impinged upon their later development.

He conveys a marvellous "feel" for Aligarh itself and the environment which was intended to become "the Cambridge of India," and in so doing he challenges again and again older crude generalizations about the nature of the Aligarh Movement. Although biography as such is not his concern, a portrait emerges here of Sayyid Ahmad Khan which does much to round off our picture of that obstinate, tragic figure, torn between the limitations of his *sharīf* background and his obsessive belief in the need to train leaders of the Muslim community who would be imbued with an Indo-Islamic variant of Arnold's ideal for Rugby. Particularly fascinating is the account of his relations with the English staff, their rather different aspirations for the college, and the striking vignette of Theodore Beck, here no sinister agent of a British policy of "divide et impera" but an impassioned Cambridge "Apostle." One is left marvelling at the way in which a detailed account of the origins of a late nineteenth-century Indian college can illuminate many of the most significant aspects of that crucial period in the history of the subcontinent.

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SUKOMAL SEN. *Working Class of India: History of Emergence and Movement, 1830-1970*. Calcutta: K. P. Bagchi & Company. 1977. Pp. 466. \$14.00.

This book is a biography of the "working class" in India from its emergence in the nineteenth century to its growth into a political force in the early twentieth century. Although written with a commitment befitting the author's activism in the trade union movement, it is sadly lacking in imaginative scholarship. As with so much labor history produced from "within the movement," the tendency here, too, is to identify the history of the working classes with that of the labor movement.

The strength of this study is its encyclopedic nature. Sukomal Sen crowds into his account a plethora of events, names, and other details, which he only punctuates occasionally by pauses for a political message. For instance, he concludes from the failures of a strike in the 1920s: "On the one side it demonstrated the undaunted spirit and ex-

cellent heroism of the workers and on the other it glaringly revealed the attitude of some of the national leaders towards the big Indian capitalists and the depth of treachery to which a reformist union was apt to droop itself" (p. 276). Here, as elsewhere, the author directs the history of the working classes to assert his Marxist beliefs. Unlike the work of E. P. Thompson or Eric Hobsbawm, Sen's version does not translate political commitment into good history.

The tone of this book is evident from the outset as it axiomatically accepts the now familiar view that British rule in India destroyed the traditional economy without replacing it with a modern capitalist economy. And as capitalism developed slowly, so did its by-product, the working class. But with the recruitment of large numbers of laborers for the construction of railways in the late nineteenth century came "the harbingers of [the] modern Indian working class" (p. 23). Similarly, the development of other industries swelled the ranks of this class, and by 1890 there were three hundred thousand people employed in factories and mines.

According to Sen, Indian laborers increasingly developed class consciousness after 1895. A number of working class struggles in the ensuing decades culminated in the formation of the All India Trade Union Congress (AITUC) in 1920. It is no coincidence that this history of organized labor in the early twentieth century reads like that of the nationalist movement, because the leaders of the Indian National Congress also played commanding roles in the trade union movement. Congress domination, however, to use the author's words, also meant that the workers' movement was channelled toward "bourgeois reformism" or "class collaboration" instead of "class struggle" (p. 223). But as Communist influence grew, the Right and the Left openly clashed, first in the 1929 session and repeatedly thereafter. In 1946 Congress founded its own labor organization, the Indian National Trade Union Congress. The book also briefly deals with the trade unions of government and other white-collar employees and organized labor since India's independence.

Whereas labor organizations and leaders figure prominently in this account, the working classes appear primarily as a backdrop. The author clearly ignores the important conceptual and methodological breakthroughs that recent labor historians have made in reconstructing the rich and subtle past of common people. Thus, there is little on the cultural life of the working class, let alone other aspects of working-class experience.

ANAND A. YANG
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TEJ BUNNAG. *The Provincial Administration of Siam, 1892-1915: The Ministry of the Interior under Prince Damrong Rajanubhab*. (East Asian Historical Monographs.) New York: Oxford University Press. 1977. Pp. x, 322. \$29.95.

This is in every way an admirable book: it focuses on an important aspect of the modern history of Thailand; it draws materials from basic archival sources; it is organized logically and written clearly.

To those unfamiliar with Thai history, provincial administrative changes in the late nineteenth and early twentieth centuries may seem a narrow topic, but this is not the case. In traditional times the territory of the Kingdom of Siam was divided into three categories—inner provinces, outer provinces, and tributary states. Only the inner provinces, close to the capital, were closely controlled; outer provinces and tributary states preserved much autonomy, including fiscal autonomy. The coming of the West, particularly France in Indochina and Britain in Burma and Malaya, posed a new threat to the Thai state. This threat was met by astute diplomacy, a willingness to concede when concession became inevitable. But just as important as foreign policy in meeting the Western challenge was domestic reform—reform to strengthen the central government, modernize it, rationalize it, increase its revenues, and enhance its control over all the territory it called Siam. Thus, the strengthening of the control of the central government over the provinces and the transformation of tributaries into regular provinces became necessary in order to save Siam. Tej Bunnag never loses sight of this vital function of internal reform.

The essential program of provincial reorganization took place from 1892 to 1915. Prince Damrong, the most powerful minister in the government of King Chulalongkorn, directed the program, called the *Thesaphiban* system of provincial administration. It involved the establishment of a hierarchy of provincial officials headed by superintendent commissioners in all regions of the country. The whole structure came under the direction of the Ministry of Interior in Bangkok.

Bunnag describes the *Thesaphiban* system in detail—its antecedents, its growth, and its transformations. What emerges most sharply from his discussion is the consummate skill of central government authorities in proceeding gradually, within the limits of a strict budget, and recognizing the need to avoid head-on clashes with entrenched local elites. This adroitness is evident in all programs of government reform conducted by King Chulalongkorn. An important contribution,

indeed, of Bunnag's study is that it enlarges our understanding and heightens our appreciation of the great achievements of Chulalongkorn.

The book is brief and always to the point. Brevity, in fact, may have been carried to a fault. One may wish to know more about the hereditary nature of traditional local rule (is the example of a Chinese governor in Songkhla typical?). What, specifically, did the "Golden Book" teach new provincial officials? How did one execute Chulalongkorn's recommendation never to allow provincial officials "to consult with each other"? Most important of all, what were the principal actors during this time of crucial change like? What sort of a person was Prince Damrong, the star of the drama? That such questions emerge is really a tribute to Bunnag's ability to engage our attention in his subject, to leave the reader enlightened yet hoping for more.

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UNITED STATES

RICHARD W. WERTZ and DOROTHY C. WERTZ. *Lying-In: A History of Childbirth in America*. New York: The Free Press. 1977. Pp. xii, 260. \$10.00.

Orthodox obstetrics is a microcosm of American medicine. As the authors of *Lying-In* point out, it is increasingly expensive, centralized, and interventionist and provides the least amount of natal care to those most in need of it, those Americans disadvantaged by poverty or racism. The sexual symbolism of the majority of Americans entering the world by way of an immobilized female at the mercy of a technologically proficient male backed by institutional authority is also apparent. How did this scene come about? Richard W. Wertz and Dorothy C. Wertz deliberately leave aside "the history of obstetrics as a science or as a profession" (p. xi). They also ignore gynecology. This is a mistake, since they want to explain "how attitudes and practices of birth have come about, how they have changed, and what they mean for patients and for health" (p. xi), and their own book suggests that dominant influences in that story have been obstetrical science and professionalism.

Another influence was the form taken by Protestantism in America, as the authors recognize in the twenty-six pages they spare the colonial period. But they neglect the Indian cultures, which could have provided the comparative dimension their subject so sorely needs. And they miss the larger possibilities offered by the case of Anne Hutchin-

son, midwife and theologian. Although they emphasize a "symbiosis of women and doctors" (p. 30) in choosing hospital delivery over home-birth with a midwife, in general, they share the revisionist view of the history of obstetrics inspired by feminism. The celebration of male heroes fighting against midwives, quacks, prudery, pain, and puerperal fever on behalf of scientific standards, and thereby conquering infant and maternal mortality, has been superseded by more balanced accounts of the midwife's work and of the campaign waged against her by the alien figure of the male expert, wielding an increasing number of instruments—many of them invented to repair the damage done by his or his brethren's hastening birth in the eagerness to master "Dame Nature," spreading the puerperal fever they later claimed to have conquered—and collecting escalating fees. Today America lags far behind other modernized nations in infant and maternal mortality.

Hospitalized, male-dominated birth was identified as a national characteristic in the face of repeated immigrant reinforcements to the midwife tradition. The authors' inadequate explanation is that midwives were poorly organized, urbanization broke up the female networks incorporating them, and women chose the putatively safer expertise of men. Unfortunately, *Lying-In*, like the small number of secondary sources on which it far too heavily relies, concentrates almost exclusively on the upper classes and on the obstetrics that became orthodoxy, claiming that even in the nineteenth century, "except among ethnic immigrants [*sic*], among poor, isolated whites, and among blacks, there is little evidence of midwifery" (p. 47). In fact, doctors did not succeed in excluding midwives until they had professionalized themselves and had coordinated propaganda and legislation during the first thirty years of this century. They did so in the face of persistent assertions of traditional sexual values and of the success of reformed midwife programs in eastern cities. That success had represented cultural convergence, not conquest.

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JAMES REED. *From Private Vice to Public Virtue: The Birth Control Movement and American Society Since 1930*. New York: Basic Books. 1978. Pp. xvi, 456. \$17.95.

The history of the American birth control movement is vital to our understanding of contemporary society. Until the publication of James Reed's

book, special pleading, ideological wrangling, and sensationalism have marred the historiography of the subject. (David Kennedy's *Birth Control In America* [1970] is an exception.) Now Reed provides a meaningful framework by focusing on significant individuals who represented changes in the birth control movement.

Although he has a concise section on birth control before Margaret Sanger, he stresses the contributions of three individuals who represented differing philosophies and methods of agitation. They were Margaret Sanger (1879-1966), who sought autonomy for women; Robert L. Dickinson (1861-1950), who advocated stable families through birth control; and Clarence J. Gamble (1894-1966), who feared differential fertility among the classes, an old concern of the eugenics movement. Generally Sanger in her later years emphasized research, Dickinson (a physician) sought better public health programs, and Gamble wanted crash programs for simple, cheap methods by the most expedient means. They often clashed over policy and strategy during their careers, but events united them in a *de facto* popular front by the end of their lives.

After all, as birth controllers, they separated sex from procreation, a notion with revolutionary implications. And, along with countless other individuals, they experienced policy limitations imposed by social context rather than lack of technology. The anovulant pill was the first absolutely new contraceptive developed in the twentieth century. Reed's account is readable and informative. The story of Katherine Dexter McCormick's financial support of Gregory Pincus, one of the key scientists who developed the pill, is an exciting account of luck, interesting personalities, and hard scientific research.

For these people birth control was a metaphor for individual responsibility and self-direction. They were middle-class reformers who wanted to extend the ideal of autonomous individualism, born in the seventeenth century and nurtured by subsequent political and industrial revolutions, to all members of society. Three massive changes in social policy aided their efforts. The first was organized medicine's support of birth control. Secondly, liberals and radicals by 1945 accepted as valid the Malthusian prediction of demographic disaster if the birth rate did not sharply decline. Finally, because of technological advances and economic uncertainty more people perceived large families as undesirable. The United States became an unlikely place for a baby boom. Concern for ecology replaced concern for the economy as economic growth, the historic basis of middle-class radicalism, lost its ideological appeal in the 1960s. As a result, birth control, the private vice of the

nineteenth century, became today's public virtue.

Although Reed discusses these ideological factors, his major concern is with individuals and institutions. As a small criticism, one must say he should have analyzed the history of ideas in greater detail, but it would have made the book too long. He mentions but does not develop the fact that birth controllers expressed the puritan notion of stewardship despite its later secularization. The concept is the essence of American reform. With the exception of Sanger, who courted upper-class support, Dickinson, Gamble, and McCormick were upper-class reformers who desired a better world. The birth control movement contributed to the intellectual process, dating from the seventeenth century, in which men replaced the "given" natural world with a "created" social world. My criticism of this lack of intellectual analysis by Reed is minor. Based on sound scholarship, his book is the best general history of birth control now available.

DONALD K. PICKENS
North Texas State University

TAMARA K. HAREVEN, editor. *Family and Kin in Urban Communities, 1700-1930*. New York: New Viewpoints. 1977. Pp. vi, 214. Paper \$5.95.

The eight essays in this volume address various aspects of United States history and represent the work of a second generation of family historians. While these scholars follow their predecessors in analyzing demographic trends and household structures, their prime concern is the relationship between the history of the family and that of the larger society. Their findings revise a number of traditional interpretations. As Tamara K. Hareven points out in a useful introduction, these studies deny the causal link between urbanization and fertility decline; question Talcott Parsons's notion of a functional fit between the nuclear family and industrial society; and dispute the thesis of urban family disorganization propounded by the Chicago School of sociology. These essays are analytic rather than descriptive and represent social-scientific attempts to determine precise causal relationships. "To what degree, under varying circumstances, was the family in control of its own decisions," the editor asks, "and to what degree was its behavior guided by external pressures and incentives?" (pp. 188-89).

The answers are suggestive rather than conclusive. In a statistical analysis of New Hampshire towns, Darrett Rutman demonstrates the intimate relationship between the density of agricultural settlement and the rate of out-migration; families apparently moved to secure land for the next generation. By 1850, however, many rural families

were achieving this goal by limiting their fertility, as Stuart Blumin shows in a comparison of rural and urban households in the Hudson River valley. Yet another wealth-preserving strategy was adopted by the Boston merchant families studied by Peter Dobkin Hall: cousin marriages and the exchange of siblings in marriages among prominent families both prevented the dispersion of wealth and created a class identity. The power of this class is then illustrated by Dean May and Maris Vinovskis; they show that elite reformers in Boston established infant schools during the 1820s to provide poor children with "proper" moral instruction. That these schools were also supported by the poor, for their own reasons, indicates that families from all social classes sought to affect their social environment. As Hareven points out, French-Canadian textile workers used ethnic and kinship ties to control access to employment in New Hampshire mills, thus expanding the economic options open to their own families. However diverse their subject matter, these five essays have a common theme—that of families struggling to determine the nature of their own existence in the face of the obstacles posed by the productive system, class power, and established modes of thought and behavior.

Two other studies focus on the life-cycle. In a joint paper the editor and John Modell suggest that the institution of boarding was not a symptom of familial disintegration but a function of the cycle of household life, with young lodgers replacing departed offspring. Laurence Glasco undertakes a comparative analysis of the life patterns of native-born whites and Irish and German immigrants in Buffalo in 1855. While male life-cycles differed economically, the lives of females were diverse with regard to work experience, fertility, and family stability. The biological rhythms of the life-cycle were not sovereign, but varied according to class, ethnicity, and gender.

Most of these essays have been previously published (primarily in the *Journal of Urban History*), and some are flawed in conception or execution. Sally Griffen and Clyde Griffen fail to demonstrate the distinctive nature of American small business, for their comparative data on French family enterprise lacks conceptual and statistical precision. Other papers neglect the emotional and psychological dimensions of family life, while Hareven exaggerates the degree of harmony between family values and factory discipline. This collection, nonetheless, indicates the complexity and the scope of family history; and it begins the task of synthesis with more traditional forms of political and social analysis.

JAMES A. HENRETTA
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WALTER J. MESERVE. *An Emerging Entertainment: The Drama of the American People to 1828*. Bloomington: Indiana University Press. 1977. Pp. x, 342. \$17.50.

Walter J. Meserve's study is now the definitive history of the American drama from the beginnings to 1828. His guidelines are simple ones: "all recorded drama written in North America prior to the establishment of the United States of America," as well as what follows. And with this declared, he proceeds to explore the drama's relationship to the "cultural and historical progress of the country," to evaluate it "as a literary genre," and to relate it to the development of the American theater. This is a lot of ground to cover, and Meserve has done it well. His scholarship is impeccable, and his book will serve for years to come, not only as background and ready reference, but also as a springboard for dissertations and other studies.

This is not to say that Meserve is the first to travel this ground. When Arthur Hobson Quinn published his *History of the American Drama from the Beginning to the Civil War* in 1923, he had already dealt with the subject six years earlier in an essay written for the *Cambridge History of American Literature* (1917), and that same year he published his anthology of produced plays, *Representative American Plays*. He also set up the first graduate work in this area in 1922 at the University of Pennsylvania and, together with Brander Matthews and George Odell, set a pattern of distinguished scholarship in the field of American theater studies. When Quinn wrote his preface to the second edition (1943) of his *History*, he was able to acknowledge the work of such theater historians as Odell, Pollock and Wilson, Hoole, Smith, and Carson, and such drama historians as Hill (*American Plays* [1934]), Firkins (*Index* [1927]), Baker (*Dramatic Bibliography* [1933]), and the first volumes of the *American's Lost Plays* series.

What Meserve has put together does not differ in kind from Quinn's approach; nor does it supersede certain aspects of Quinn's work, especially his splendid bibliographies, which are still highly useful. It does differ in its length and breadth. By terminating his study at 1828, versus Quinn's 1860, he is able to go far deeper into the minor drama than Quinn ventured and to give expanded coverage to what both he and Quinn consider major. Also, by being able to take advantage of a number of studies in American drama/theater published since Quinn's second edition, he has been able to show a richer texture and a more specific evolution in the early American drama and its relationship to the theater. With this much more detailed focus, he is able, for example, to show the dramatic significance of the early Indian ceremonies in treaty-

making, as well as the effects the exploration and conquest of America by the French, Spanish, and English had on the beginnings of native drama. The depth of the coverage is apparent when Meserve's list of more than four hundred titles mentioned in the text is compared with William Dunlap's assertion in 1932 in his *History of the American Theatre* that America up to that point had produced some two hundred and seventy-five plays, including translations and some unperformed works. This is significant in the total evaluation.

What makes *An Emerging Entertainment* an up-to-date authority is its breadth of treatment, its careful scholarship, and its specific attention to interrelationships. Meserve's book provides the appropriate historical overview to accompany the early works published in the *America's Lost Plays* series, which the Princeton University Press began in the early 1940s and the Indiana University Press continued and republished in the 1960s. The latter is certainly to be congratulated for its unusual efforts in making available not only the collected play series but also this volume and other studies in American theater/drama.

FRANCIS HODGE
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Austin

CARL DIEHL. *Americans and German Scholarship, 1770-1870*. New Haven: Yale University Press. 1978. Pp. xi, 194. \$15.00.

In this book Carl Diehl argues that at the end of the eighteenth century the humanistic vision and historical imagination of German scholars began to lose ground before the rising importance of historical and philological research technique. A new concept of scholarship emerged in which the asking of questions considered to be answerable in scholarly fashion and the defining of a "field" of specialization became the crucial criteria. It was this victory of research technique over humanistic vision and historical imagination that constituted the crisis of historicism which haunted European scholars.

American students who studied philology and history in German universities before 1870 lived through this transformation as participants. Diehl maintains that they remained curiously out-of-tune-and-touch with the significance of what they were witnessing. They acquired the research techniques, rejected or could not grasp the humanistic vision, and remained oblivious to the crisis of historicism. Why? Diehl does not pretend to have the answer. But he suggests that for the first generation of American students in Germany between 1815 and 1840 the reasons were psychological and

difficult to plumb. He finds them easier to see for members of the second generation of American scholars abroad after 1840. Their practical spirit aroused, the Americans seized upon methodology and technique as the most salient attributes of the profession of research. Research, writes Diehl, "was less a religion than a business practice to them" (p. 151). When these young Americans returned home as hopeful academic entrepreneurs, they committed themselves to specialization and research and set about to develop the academic profession. From their bequest of German scholarship they selected vocation and ignored vision.

Diehl's book represents a refreshingly different approach to a topic known to us previously through the works of Orie Long and David Tyack, of Alfred Faust and Henry Pochmann. Diehl deliberately shifts emphasis from the general but vague recognition of cultural influences to the problematic tension which he sees existing between the scholars' commitment to *Neuhumanismus* and national revival through literature and their specialized philological scholarship. His admirable exploitation of this theme benefits from his illuminating analysis of German philological scholarship—especially the work of Friedrich August Wolf—and makes the book a welcome addition to our literature on this subject.

Less satisfying is Diehl's relative neglect of the world of the antebellum college in America, of the dominance of moral philosophy and its meaning, and of the status insecurities of American academic men with their ambivalence towards academic professionalism. Not exploring these aspects of American experience, Diehl finds it difficult to explain the psychological difficulties he attributes to the first generation of American students in Germany. Comparative history demands equal attention to the societies, cultures, and institutions to be compared, and Diehl is obviously less familiar with American than with German educational history.

Even so, the book makes a real contribution. Diehl has something to say, and he says it well. But just what does he imply, I wonder, when he writes that something may be true "for the last ten decades of the nineteenth century" (p. 51)?

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JOSEPH A. WYTRWAL. *Behold! The Polish-Americans*. Detroit: Endurance Press. 1977. Pp. xi, 667. \$15.00.

When Joseph A. Wytrwal's first book, *America's Polish Heritage*, was published in 1961, it was the closest approach to an adequate general history of

Polish-Americans yet written. Unfortunately, his subsequent volume, *Poles in American History and Tradition* (1969), was largely a filiopietistic listing of every Polish-American who ever fought in a war or got his name in a newspaper. Now he has produced a third attempt to survey Polish-American history, from the background in Poland, through the settlement and organization of Polonia in America, up to the so-called ethnic revival today. *Behold! The Polish-Americans* is, however, more an encyclopedic rehash than a new synthesis, a settling of old scores rather than a mature second thought. Twelve of the twenty-three chapters in this book are taken bodily from his two earlier volumes. In addition, there is new material: a brief, charming chapter on Polish-American folk customs, some perceptive comments on generational conflict, and yet more lists of famous Polish-Americans somehow missed in his previous books. Throughout the book the author attacks the real and imagined enemies of the Poles, from Franklin Roosevelt at Yalta to the *It's Fun to Be a Polak* jokebook.

The most striking new material, however, deals with the relations between Polish-Americans and other ethnic groups. A chapter entitled "The Polish-Irish Encounter" is an extended diatribe against Irish domination of the Catholic Church hierarchy. The Irish bishops were, according to Wytrwal, "fickle and unstable . . . noted for their ambition, truculence, quickness to take offense, and revengefulness" (p. 183); also they were "opportunists and fawners" (p. 185). The author ignores much recent work on ethnic tensions in the Catholic Church and bases most of this splenetic attack on a twisted reading of John Tracy Ellis's thirty-year-old biography of Cardinal Gibbons. The two chapters on Polish-black relations and Polish-Jewish relations attempt to prove that Poles and Polish-Americans have always loved their black and Jewish brethren and have been unfairly maligned for prejudice. The author produces more lists as evidence: Kosciuszko freed black slaves, Paderewski was generous to a black servant, and Andrew Young has a Polish ancestor. Yet at the end of the chapter on blacks, the author affirms that Polish benevolence has not been returned. "So far the Blacks . . . have failed to deliver to the Polish-Americans on any issue" (p. 496). Wytrwal is extremely exercised by the frequent charges of Polish anti-Semitism. He chronicles in great detail the many Poles who risked their lives to hide Jews from the Gestapo during World War II. But his own peroration to Polish-Jewish relations goes as follows: "The Jew's selfish outlook at the world will continue to reap a harvest of bad seed until their egomaniacal concept 'of the chosen people' is humanized . . ." (p. 523). Compared to this, the

brief chapter on Polish-Indian relations is harmless, but silly.

Wytrwal has combined the worst of the old and the new ethnic history. He mingles the mindless trivia of the filiopietists with the fashionable bigotry of the "new ethnicity." This book may have some value as a reference work (it has an extensive index), but it has more as an example of how not to write ethnic history.

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PHILIP S. FONER. *American Socialism and Black Americans: From the Age of Jackson to World War II*. (Contributions in Afro-American and African Studies, number 33.) Westport, Conn.: Greenwood Press. 1977. Pp. xiv, 462. \$22.95.

The prolific pen of Philip S. Foner has now produced a long-overdue monograph on the history of American socialism. *American Socialism and Black Americans* deals with the important cultural issue of the relationship of American dissenters to this society's most oppressed minority. Foner, unlike many historians of American socialism, begins his study in the antebellum years with a consideration of the earliest socialists in the United States, most of whom were Germans. Tracing the careers of the little-known Carl Heinrich Schnauffer, Carl Daniel Adolf Douai, Wilhelm Weitling, and, the best known among them, Joseph Weydemeyer, while omitting only Fritz and Mathilde Giesler-Anneke of the socialist Forty-eighters, he presents their views on black emancipation and equality. Thereafter, the volume evolves into an institutional study and examines the Socialist Labor Party and especially the Socialist Party of America. It merely touches upon the emergent Community Party, essentially leaving that history for a second volume.

The book draws upon most of the socialist press (except for an occasional omission, such as the *American Socialist*), a scattering of correspondence, and most recent secondary works. It casts a wide net as it seeks to encompass a greater range of topics than the surveys of American socialism, such as David Shannon's. The author includes, for example, chapters on socialist racial practices in the Deep South; policies in the Southwest, where the Oklahoma socialists led a vibrant movement; party ties to the National Association for the Advancement of Colored People; and socialist activity in Harlem. Consistently, the author emphasizes the contrast between party pronouncements and party practices. This comprehensive approach fleshes out the reader's understanding of the relationship of socialism to blacks, but it does not

suggest a new thesis. Rather, it shores up an existing one. In the 1950s Ira Kipnis emphasized what he called an increasing racism among American socialists in the early twentieth century, and in the 1960s James Weinstein exaggerated an awakening recognition by the Socialist Party of the magnitude of black exploitation in this country. But the thesis now accepted is that most Progressive Era socialists were racist in outlook, while a few among them, including the black contingent, were egalitarian. The minority challenged the fundamental party position that the class issue was dominant over any racial or nationality question. Foner's study details the tenacious hold of the class analysis on most party members, but we do not learn as much as we might. The reader would be better served had the author utilized the correspondence of all the major party figures to delineate differences of degree in their racial attitudes. Indeed, the author's omission of such specifics somewhat undercuts his contribution.

Foner's important discussion of black socialists focuses on party activists who have been almost entirely neglected by historians. Thus, in tracing the careers of socialist preachers George W. Woodbey, George W. Slater, Jr., and George Frazier Miller, as well as the better-known A. Philip Randolph and Chandler Owen, editors of the Harlem-based socialist magazine, *The Messenger*, Foner fills a serious gap in the literature. But the author does not convincingly demonstrate the influence of these individuals who attempted to align their black constituents and their own socialist cause.

In furthering his argument of socialist superficiality on racial issues and lack of serious commitment to equality, Foner indicates that between 1901 and 1918, the "golden age" of the Socialist Party, no resolutions on black Americans were passed and no agitation was promoted to undermine second-class citizenship. Even the humanitarian Eugene V. Debs did not recognize the dimensions of the black plight. During World War I, the Socialist Party displayed greater sensitivity to black exploitation but quickly reverted to its narrow approach to race. The author rightly maintains that, while the Socialist Party principle of equal rights regardless of color, race, or sex was advanced for its time, the party was weak in such policy implementation and, indeed, in vision. He concludes that the party thus limited its chances of winning black converts, but he fails to consider environmental reasons external to the party that might have minimized its attractiveness to black Americans. More than party deficiencies need to be considered, for the socialists and their relationship to blacks cannot be studied in a vacuum. Reasons for the aloofness of black workers toward the Socialist Party must be explored in a wider

frame of reference. Nevertheless, while some questions remain to be explored, this monograph will now assume a place in the basic literature of American socialism.

SALLY M. MILLER
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THOMAS C. LEONARD. *Above the Battle: War-Making in America from Appomattox to Versailles*. New York: Oxford University Press. 1978. Pp. viii, 260. \$12.95.

In *Above the Battle* Thomas C. Leonard examines how Civil War participants, and the following generation of Americans, thought about enemies, weaponry, and the violence of combat. The result is a book of rich, multilayered analysis and brilliant suggestiveness.

Civil War veterans, he argues, though they had experienced in combat the emotional gamut from exhilaration to horror, chose to idealize the war and to insist that it had been a humane, efficacious, and comprehensible conflict. Reticence regarding combat's tragic elements was the price of sectional and personal healing, and as a result the heroic pose remained intact. Nor was it to be shaken by the appearance of new weaponry, for soldiers and scientists assumed that the more formidable the weapon, the greater would be its power to reduce slaughter, or indeed to end war. Even short of this anticipated deterrence via decisiveness, technological advance would enhance rather than diminish, or so many thought, latitude for individual heroism. Even peace groups supported stoicism in battlefield reportage and encouraged the search for the decisive weapon. Such formulations, Leonard suggests, set in train numerous problems. Since former Confederates so quickly again became comrades, the soldier's stance vis-à-vis the enemy was one of respect, a source of confusion in confronting new foes. He felt little hatred of the Plains Indian, the Spaniard, the Filipino insurgent, or even the German enemy of 1917-18; "vital questions about shame and guilt [had] disappeared" (p. 36). American perceptions remained focused not on bloodletting but above the battle, on war as a game of strategy and tactics or as a valuable instruction in discipline and character. Only in the 1920s did Americans begin to concede that war might produce pain without redemption, force without mitigation, and violence of a scale preclusive of individual comprehension or meaning.

Leonard's is a broad and often original formulation whose analytical texture resists the easy summarization attempted here. He brings integrative skill of a high order to wide-ranging memoir and literary sources, though at points he places on the

Civil War legacy more weight than it can support. To it, for example, he attributes the troubling doubts created by the respect that professional soldiers felt for their Indian foes. Some frontier commanders, to be sure, grew to admire Indian courage and resourcefulness, but Sherman and Sheridan, equally legatees of the Civil War experience, scarcely budged from their vision of blood-thirsty savages deserving of obliteration, man, woman, and child. (Indeed, in Sherman's reminder to his Plains commanders that Northern artillery did not cease shelling Vicksburg and Atlanta because there were noncombatants present, Civil War precedent may intensify rather than mitigate harshness.) The distinction is that between front- and rear-echelon, the determinant was less a view of the enemy forged in the Civil War than the propensity of those in close contact to feel a professional respect for a valorous antagonist. Other issues deserve more rigorous exploration. Some of those categories which he seems to draw from his respect for the work of J. Glenn Gray—guilt and shame, for example—are insufficiently grounded in social and political process, and one does not know whether their import derives from developments peculiar to the period or from assumed patterns of universal psychological compunction.

No such reservation, however, dims the fact that Leonard attempts much and succeeds in much. Indeed, the last of the book's virtues is that it is written in a manner to invite such questions, to solicit further explorations of the issues he so well opens to us.

GERALD F. LINDERMAN
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MORRELL HEALD and LAWRENCE S. KAPLAN. *Culture and Diplomacy: The American Experience*. (Contributions in American History, number 63.) Westport, Conn: Greenwood Press. 1977. Pp. x, 361. \$19.95.

In this blend of intellectual and diplomatic history, Morrell Heald and Lawrence S. Kaplan discuss the influence of the American "pattern of culture" on the foreign relations of the United States. They distinguish three features of this pattern as particularly important in American diplomacy: a sense of mission, the pursuit of opportunity, and commitment to limited government. In essays on topics ranging widely in time and in focus (Franco-American relations, 1775–1815; British-American relations, 1783–1823; relations with Latin America and China in both the nineteenth and twentieth centuries; and relations with Israel, India, the Soviet Union, and Africa in the twentieth century, to

name some of the topics), the authors consider the effects of these features, especially in terms of the interests and activities of private citizens and groups, in American foreign affairs. For Heald and Kaplan argue that the American pattern of culture holds particular importance in foreign affairs because of the ability of nongovernmental individuals and groups to shape United States policy and indeed to conduct a substantial portion of the nation's business abroad.

Though of mixed quality, the results of this unusual enterprise deserve reflective reading, and not only by diplomatic historians. One reason that the results seem mixed is that in several chapters the book's main themes fall from view. Another reason for an impression of uneven results is that the chapters vary considerably in the proportion of narrative detail and interpretation. Yet the book displays strengths more significant than its difficulties. The authors command an elegant prose style, in itself rare enough to be noteworthy. Most of all, the book is distinguished by mature judgment, backed by sure, broad knowledge of American foreign affairs. The result, happily, is a book studded with insights on familiar topics and issues of American diplomacy, a book to contemplate as well as to read.

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DANIEL J. KEVLES. *The Physicists: The History of a Scientific Community in Modern America*. New York: Alfred A. Knopf. 1978. Pp. xi, 489. \$15.95.

This long-awaited book about the profession of physics in the United States since the 1870s has been worth the wait. It is an excellent study of the professionalization process as well as of the most basic social, economic, and political aspects of the practice of the science of physics over the past hundred years. It purports to be "an unabashedly interpretive history" of the leading individuals and institutions that shaped the modern discipline, but it also presents a narrative blend of intellectual achievement within social constraints and of personal influences as well as statistical inferences. Daniel J. Kevles has synthesized a wide variety of materials to tell well the story of the maturation of physical science in America.

Beginning with the post-Civil War era of reconstruction in science, Kevles proceeds chronologically with a chapter for each decade before World War I. He systematically characterizes the main trends of each period. Building upon the dissertation he presented in 1964 and upon numerous special studies he has published since then, Kevles

here amplifies the main-line story of the development of a large-scale community of American physicists. Educational policies, research priorities, primary achievers like Joseph Henry, H. A. Rowland, A. A. Michelson, and J. W. Gibbs, and the problems of patronage and institutional reform occupy center stage through the turn of the century. But Kevles also stresses the sideline of minority problems within physics: why women, Catholics, and blacks had such difficulties in physics compared to Jews and Orientals, for example.

Tracing the growth of industrial research laboratories and philanthropic influences, like those of the Carnegie Institution of Washington and the Rockefeller Foundation, Kevles charts the public and private lives of physics as a young profession through the first half of the twentieth century. Beyond the relativity and early quantum revolutions, he shows how "invisible colleges" developed to raise standards and to gain and distribute funds. The entrepreneurial activities of George Ellery Hale and Robert A. Millikan, especially during and after the "Chemists' War" of 1914-18, led to the defeat of Edisonian empiricism and to a close alliance with governmental and military patrons. Growing beyond the eclectic American Association for the Advancement of Science and the moribund National Academy of Sciences, physicists were instrumental in setting up the National Bureau of Standards, the National Research Council, and the American Institute of Physics, as well as other agencies more responsive to their and the nation's changing needs. After the advent of quantum mechanics in the late 1920s and particularly the "miraculous year" of 1932—when the confirmation of the neutron, the identification of deuterium, and the verification of the positron all came together to give birth to the new field of nuclear physics—a new sense of excited enterprise arose. Despite the Great Depression and in part because of the persecutions in Nazi Germany, American physics in the 1930s began to achieve a balance between experimental and theoretical expertise which led quickly to a shift in the professional center of gravity to these shores. Kevles tells this story in an admirably clear and trustworthy fashion.

Over one-third of the book is devoted to the period since 1940. The dramatic stories of the "Physicists' War" of 1941-45, the development of nuclear weapons and establishment of the Atomic Energy Commission, the victory of best-science elitism in the founding of the National Science Foundation, for instance, and the new revolt against science of the 1960s are all well formulated. Although Kevles skirts the growth of the hybrid sciences like astrophysics, biophysics, and physical chemistry, he focuses on his prime target—the

professional community—and he carries through to his final page the theme of conflict between the elitist quest for excellence and the egalitarian search for a politically responsive ethics and equality of opportunity.

Kevles's history provides a model of what is needed now for the history of American chemistry and biology. Read in conjunction with Thomas L. Haskell's *The Emergence of Professional Social Science* (1977), Kevles's book presents a sure guide to the background of the multiple crises of authority in our century.

LOYD S. SWENSON, JR.
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C. HUGH HOLMAN. *The Immoderate Past: The Southern Writer and History*. (1976 Lamar Lectures.) Athens: University of Georgia Press. 1977. Pp. ix, 118. \$7.00.

This study of how southern novelists have used and abused their past is valuable to the historian and the student of literature. By analyzing antebellum attempts to understand the past in William Gilmore Simms's Revolutionary War romances, the dissection of a lost past in the post-Civil War realistic novels of Ellen Glasgow's *Virginia*, and the tormented postrealistic twentieth-century guilt and foreboding of Robert Penn Warren and William Faulkner, the author has succeeded in revealing the importance of the past for much of southern fiction. In no way aspiring to originality, since many of the same themes have been developed more cogently and more fully by other critics, C. Hugh Holman has been content to show how obsession with the past has affected the literary qualities and intellectual messages in the works of many Southern authors.

In his fine synthesis Holman at times confuses what history meant to Southern writers and neglects important aspects of their fascination with the past. He has overstated his conclusion that most southern novelists used history to understand the present and the future. Many southern writers have actually used the past to avoid antisouthern tendencies in the present South. By emphasizing the orderly progression of events, Holman neglects southern writers who described people who often repeat mistakes. He dismisses the frontier experience of the South and ignores the way writers have included spatial and geographic movement over time in their obsession with the past. He does not comment on Simms's border romances, which were set in the recent past and which captured the relationship between the disruption of past values and the sense of renewal that derived from pushing back the wilderness. Faulkner also wrote of the

semifrontier of Mississippi, comparing that early wilderness society with the old values of the Southeast and those of the modern, settled, and civilized Southwest.

With the mention of Cleanth Brooks's brilliant analysis of Faulkner's warning that the past is no place to hide, Holman has provided lessons for all historians who would face the realities of the past. The author remains essentially optimistic over what southerners learned from the past to use for the future. But many regions of the South had such a dark past that not a few southern novelists and historians came to believe that the burdens of southern history are too great to overcome.

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KENNETH COLEMAN, editor. *A History of Georgia*. Athens: University of Georgia Press. 1977. Pp. xvi, 445. \$12.50.

Six members of the history department of the University of Georgia have written this new state history under the general editorship of Kenneth Coleman. Covering four centuries, from the time of Spanish missionaries in Guale to the present, this work is the best and most up to date in print. Each historian was responsible for a particular period: Phinizy Spalding, the colonial era; Kenneth Coleman, 1775-1820; F. Nash Boney, 1820-65; Charles E. Wynes, 1865-90; William F. Holmes, 1890-1940; and Numan V. Bartley, 1940-77. The pitfalls of a collaborative work of this nature are innumerable. Coleman is to be commended for keeping repetition to a minimum and, though some sections read more spritely than others, for having a uniform, smoothly flowing narrative. Each author, an expert in his area, effectively synthesizes recent secondary literature and draws on his own research.

Since the colonial period is longer than the others combined, Spalding's assignment in some respects is the most difficult. He not only adeptly recounts Savannah's founding in 1733 and the tribulations of Oglethorpe and the trustees but also considers the earlier Spanish period. Franciscans labored and suffered martyrdom to proselyte coastal Indians. If this book serves no other purpose, it may inspire the reconstruction of one of these missions, which even better than this work can interpret to the general public a neglected century of Georgia's history. Before the Revolution—and for some years afterward—Georgia, having only a small population, remained a frontier colony or state. As a result, for over two and one-half centuries Indian relations were of considerable, and at times overriding, importance.

Political and economic trends are emphasized

throughout. For decades after the Revolution political factions or parties existed, and the frontier heritage was manifest in rough and tumble political strife. Button Gwinnett, signer of the Declaration of Independence in 1776, was killed the following year in a duel, and Alexander H. Stephens, future vice president of the Confederacy, was nearly done in by a bowie knife in 1848. Coleman is undoubtedly correct in arguing that personalities more than issues separated parties, but to label most Georgians in the 1790s as Federalists, considering their support of Citizen Genêt, is an oversimplification. Boney convincingly argues that the *Gone with the Wind*, fire-eater stereotype of the antebellum white is misleading. Georgians refused to follow South Carolina's leadership in the 1832 nullification controversy; only 2,500 bothered to vote to send delegates to the Nashville Convention; and in 1860 they were still badly divided. Georgia was not the first but among the last of the lower South states to secede. In part because of her frontier-rural background, Georgia's support of public education was minimal until the twentieth century. More often than not, artistic and literary figures achieved prominence in spite of, rather than because of, the educational system.

Until well into the twentieth century, the black population almost equaled that of whites. Wynes deftly analyzes white and Negro participation in Reconstruction and the subsequent evolution of sharecropping, convict leasing, and Bourbon dominance during the Gilded Age. It is paradoxical that not until the Martin Luther King era, when blacks constituted a far smaller percentage of the population, did at least some of them emerge as distinct personalities rather than ciphers. Bartley's section on the modern period is most valuable. For the first time Georgia's history is brought up to date, and one can follow the passing of the county unit and Gene Talmadge era and the emergence or dominance of urban centers, primarily Atlanta. The evolution of Atlanta from a nineteenth-century railroad junction to a twentieth-century megalopolis is a remarkable story.

Scholars will compare this state history with its predecessor by E. Merton Coulter, which has served as the standard since 1933. Coulter placed little emphasis on the contributions of blacks, the Spaniards, and some major twentieth-century developments. He devoted much attention to the British colonial and antebellum eras, and these sections can still be read with profit.

Governor Jimmy Carter commissioned this new history in 1973, and four years later President Carter wrote a foreword to the completed work. Much has transpired between the first arrival of Franciscans on St. Simons Island and President Carter's visits there for relaxation and the planning of gu-

bernatorial and presidential strategy. Georgia has always had a mixed ethnic population of diverse Christian and non-Christian beliefs. Though separated by four centuries, the opportunities and pitfalls of a zealous Franciscan and a born-again Baptist symbolize, to an extent, the promise and anguish of Georgia's—and the nation's—long history.

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PETER GREGG SLATER. *Children in the New England Mind: In Death and in Life*. Hamden, Conn.: Archon Books. 1977. Pp. 248. \$15.00.

In *Children in the New England Mind* Peter Gregg Slater focuses on the concept of infant depravity and observes that "contemporary perspectives on child rearing are not as new as is commonly believed" (p. 127). In the years between the founding of the Bay Colony in the seventeenth century and the emergence of romanticism in the nineteenth century, Americans living in New England changed their minds about the nature of children. Seventeenth-century Puritans considered children "twisted toward evil," but New Englanders who lived in the eighteenth century, and who had been influenced by the writings of John Locke, regarded children as "totally plastic." By the nineteenth century American romantics could regard children as "inclined to the good" (p. 10).

Ironically the seventeenth-century Puritan view may have been the most beneficial for children, for Slater argues that "in a way depravity humanized children and made it easier to relate emotionally to them as beings who were as morally flawed as adults" (p. 23). Thus the old stereotype that the Puritans saw children as miniature adults might now be modified to read that Puritans saw children as fellow human creatures, with similar weaknesses. The romantics, by contrast, endowed children with "such ineffable goodness" (p. 23) that they worshipped children instead of treating them as members of the human race.

Children in the New England Mind consists of four "semi-independent essays"—"Parental Bereavement and the Shadow of Infant Damnation in Puritan Society," "The Disappearance of Infant Damnation," "The Child Psychology of Timothy Dwight," and "The Structure of Child Rearing Theory; Systems of the Early National Period." This slightly unusual structure, while the cause of some repetition, helps to clarify Slater's focus on the course of the idea of infant depravity over a period of nearly two hundred years. In tracing infant depravity, Slater discovered that it was an elusive concept, "like an insane relative pacing the

attic in a nineteenth century novel, hardly mentioned by the characters yet throwing a pall over their lives" (p. 17). The principal figures in the Puritan discourses about infant depravity were Michael Wigglesworth, whose well-known 1662 poem, *Day of Doom*, contained the only full discussion of the idea of infant depravity, and Jonathan Edwards, whose formulations of the nature of sin provided Calvinists with answers to the challenge of John Locke's ideas.

Using historical sources as well as psychoanalytical insights, Slater argues persuasively that Puritans' attitudes about children need to be re-examined in the light of current sensibilities. Unlike some psychohistorians, Slater has not used psychoanalytic theory as an Aristotelian logic machine producing certainties despite a lack of evidence. He is to be commended for his restraint and for the quality of his insights. While the research is both sound and thorough, and the questions raised both timely and well put, the work is finally, disappointingly brief. Nevertheless, *Children in the New England Mind* is a major work, and no historian of the Puritans or of children can afford to ignore it.

JOSEPH M. HAWES
Kansas State University

PETER BENES. *The Masks of Orthodoxy: Folk Grave-stone Carving in Plymouth County, Massachusetts, 1689-1805*. Amherst: University of Massachusetts Press. 1977. Pp. 273. \$20.00.

PETER BENES, editor. *Puritan Gravestone Art*. (The Dublin Seminar for New England Folklife, Annual Proceedings, 1976.) Boston: Boston University and The Dublin Seminar for New England Folklife, Dublin, N.H. 1977. Pp. 142. \$5.95.

Colonial gravestones have enjoyed a new lease on life in the last quarter of a century. As the two books under review bear witness, scholars from several disciplines, as well as photographers, artists, historic preservationists, and rubbings enthusiasts now devote meticulous attention to these stones and their carvings. Modern interest in gravestone art commences with the 1927 study of Harriette M. Forbes, *Gravestones of Early New England and the Men Who Made Them*, which demonstrated that the colonists carved and did not import their stones. Not for another quarter of a century did the book begin to find its audience and the subject its students.

The catholicity of these students is evident in the range of participants in the conference held at Dublin, New Hampshire on June 19 and 20, 1976, whose papers or tape-recorded remarks comprise the volume titled *Puritan Gravestone Art*. In the open-

ing address, James Slater applies his expertise as an insect taxonomist to the techniques of identifying the styles and production of individual carvers, as a necessary prerequisite to interpreting the symbolism and imagery of their art. William Harding then analyzes the style of an early nineteenth-century carver in southern Vermont. Other papers deal with literary, archeological, legalistic, and bibliographical approaches to gravestone studies, and one group considers the technical and preservationist aspects of rubbings. A spirited and unresolved discussion in one forum weighs the injurious versus esthetic effect of lichen on the stones. In a breezy presentation, Ann Parker and Avon Neal describe their experiences persuading museum curators of the artistic and accessional value of gravestone rubbings. A highly technical paper by archeologists Frederick Gorman and Michael DiBlasi defends seriation analysis—an attempt to devise a chronological sequence for artifacts—from the criticism that certain nonchronological sources of variation interfere with the analytic procedure. The concluding bibliographical essay by Nancy Buckeye, “Early American Gravestone Studies, The Structure of the Literature,” documents what this volume of papers clearly reveals, namely the amorphous character of publication and presentation on colonial and early national gravestone art.

The main interest for the historian lies in a group of four provocative theoretical papers. The religious historian David Hall flatly rejects any connection between gravestone folk art and formal religious discourse, as championed by Allen Ludwig and Peter Benes, leading scholars of the subject. The art historian Stephen C. Foster similarly rejects the connection between the religious and political beliefs of the carvers and their art. Ludwig, also an art historian, and Benes, a cultural historian, then present essays advocating the theories challenged, and one wishes the conference had been structured so that they could have responded to their critics. In their books, *The Masks of Orthodoxy*, by Benes, also under review here, and *Graven Images*, by Ludwig, as in their papers, they contend strenuously that Puritan iconography reflects Puritan theology.

Historians clearly have a stake in this subject. Gravestones and their carvings are one kind of cultural artifact surviving from the colonial period. The town cemetery may prove a complementary resource to the library and the archives. What can these long-neglected stones tell us about their obscure carvers, their patrons, their viewers, their subjects? Peter Benes, in a work of relentless scholarship, *The Masks of Orthodoxy*, advances the thesis that the symbolism of the stones can reveal a good deal about the folk-religious conceptions of

Plymouth County congregations in early Massachusetts, and by extension about the rest of Puritan New England. Benes used for his data base 4,000 photographs of single gravestones taken between 1968 and 1972 and employed every possible ethnohistorical resource to identify the carvers and relate their art to their local historical and religious circumstances. He addresses himself to three propositions: one, that Plymouth County carvers intended their skull images not as death symbols but as symbols of ghosts and spirits freed by death; two, that the carvers deliberately represented grace, resurrection, and salvation through facial caricatures, such as smiles; and three, that these caricatures derived from a Puritan expressive folklore, in contrast to conventional baroque iconography.

To prove these points, Benes provides sequential drawings of scowling skulls developing smiles and ultimately yielding to angel faces. These shifts occurred, he contends, when the carver and his community felt confident of the state of grace of the deceased, and presumably of the congregation of saints. In troubled times, such as the decade 1680–90, when witches, Indians, and the Church of England harassed New England, carvers depicted anxious expressions on the skulls.

In his argument, Benes relies heavily on the concept, written into his subtitle, that this gravestone carving was a folk art. He writes at the outset that “a final assumption on which the accuracy of this study depends is that gravestone carving was begun and maintained for at least seventy years in Plymouth County as an indigenous folk practice” (p. 8). Benes contrasts “rural-folk” with “urban-commercial” styles (p. 94) of Newport and Boston and relates how a “terse and witty folk art deteriorated into an urbane, commercial art” (p. 134).

His thesis rests on the premise that this gravestone folk art reflected Puritan religious beliefs. Hall challenged this premise in his essay, “The Gravestone Image as a Puritan Cultural Code” in the Dublin Seminar volume while appreciating Benes’s “intense effort to link particular cemeteries, particular communities, particular carvers, to particular religious groups” (p. 28). Hall observes that a scholar in one field trying to master another goes to a leading textbook and quotes from it a passage that necessarily oversimplifies a complex issue. This is what the art historians and archeologists trying to relate Puritanism to gravestone imagery have done. Because Puritanism and the stones both exist in the seventeenth century, they must be connected, says the Ludwig-Benes school. Hall disagrees.

As a folklorist, I completely agree with Hall on the futility of much cross-disciplinary synthesis. In trying to define criteria for folklore, Benes turns to

an authority for guidance and comes up with this criterion: "The single most important condition for the development of a folk culture is probably isolation . . ." (p. 8). Well, it is not. This is the old-fashioned, dying and about-to-be-buried concept of folklore. Today, folklorists look to the cities, to modern life, to the automobile, and to the factory for their data. The chief subscribers to, and collectors of Puritan folklore, were elitist intellectuals in Boston, Increase and Cotton Mather, who recorded illustrious providences. Colonial gravestone art may well be a folk art, but it is not so because Plymouth Colony was isolated. By Benes's own evidence, the imagery seems common to much of New England, including urban centers. The citizens of Boston believed in ghosts and witches as much as did the residents of Scituate and Middleborough. Nor did colonial New Englanders view ghosts and the spirit world in a positive light. Gravestone folk art, like the folklore, reflects the overriding theological concern of the Puritans with salvation and damnation, but the folk art does not embody the oral folklore.

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SYLVIA DOUGHTY FRIES. *The Urban Idea in Colonial America*. Philadelphia: Temple University Press. 1977. Pp. xviii, 218. \$12.50.

Sylvia Doughty Fries pushes the study of American attitudes toward cities back past its usual focus on the nineteenth and twentieth centuries to an examination of the colonial era. She concentrates on the early years of Boston, Philadelphia, Williamsburg, and Savannah, which shared the characteristic of being planned English communities, and draws her evidence from the formal designs of these cities as well as from statements by their founders. Fries contends that most Americans were from rural English backgrounds and saw the rise of cities like London as a threat to the social and economic order they knew. These early Americans hoped that the cities of the New World would be instruments for reconstructing the old order of the countryside.

Within her general theme, Fries finds considerable variation. The Puritans hoped to create a worldly manifestation of Augustine's City of God or the New Jerusalem, and, indeed, John Davenport consciously modeled New Haven after the towns described in the Old Testament. William Penn likewise hoped to create a city that would be an extension of a traditional rural society, based on individual ownership of land, a consensually accepted social hierarchy, and a common religious

conviction. The founders of Williamsburg, however, were influenced by the modern notion that the city was the linchpin of an urbane society. Fries argues that the Virginia gentry, insecure in its new wealth and deprived by slavery of the moral satisfaction of self-sufficiency through husbandry, used Williamsburg as the stage on which to act out its roles of political and cultural authority. The Virginia gentry's creation, unlike Boston and Philadelphia, was planned as an esthetic statement of civic grandeur and followed closely the tenets of architectural symmetry familiar in Renaissance and baroque design.

Fries calls Savannah the most distinctive American city. James Oglethorpe, she argues, was a man of the eighteenth century, who found the basis of civic virtue not in religion but in a well-balanced constitution. His goal was to restore a harmonious social order in which each citizen would possess an amount of land that would allow him or her to be independent and to develop the capacity for moral conduct and public virtue. As a result, Oglethorpe planned a unique integration of country and city in Savannah with each resident having not only a garden adjacent to his house but also a substantial farm on the outskirts of the town.

The Urban Idea in Colonial America is a provocative book but also one with problems. The author's use of the colonial attempt to preserve or restore rural values in an urban setting as the unifying theme camouflages the amount of variation and change that was present. She successfully illustrates one idea underlying the American city; she does not convincingly show that it was the major one. Boston, Philadelphia, Williamsburg, and Savannah all deviated from their founders' expectations. If historians hope to understand the colonial city, it seems necessary that they should examine not only abortive theories but also the stated and unstated ideas to be found in the realities of both unplanned and planned urban areas.

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MARION TINLING, editor. *The Correspondence of the Three William Byrds of Westover, Virginia, 1684-1776*. In two volumes. Foreword by LOUIS B. WRIGHT. Charlottesville: University Press of Virginia, for the Virginia Historical Society. 1977. Pp. li, 442; xxv, 443-859. \$32.50.

It was only fitting for Louis B. Wright to pen an introduction to this fine edition of the Byrd letters since both Wright and editor Marion Tinling have labored so long and successfully to present us with the full record of one of the most remarkable fami-

lies of colonial America. In the course of their editorial work on the diaries of William Byrd II, Tinling and Wright sought out extant letters of the Byrds. Wright's various other activities—including the preparation of *The Prose Works of William Byrd II*—prevented him from sharing completely in this enterprise. Consequently, it was Tinling, with her customary skills, who brought this final Byrd project to fruition.

Tinling provides us with all known Byrd letters, except for a small number written by William Byrd II, which are already available in *Another Secret Diary* or in Byrd's *Prose Works*. *The Correspondence* is divided into three sections, each beginning with a biographical sketch of the Byrd in question followed by the letters (incoming and outgoing), with proper annotations.

The chronological distribution of the letters may come as a pleasant surprise to scholars since we might well have logically assumed that the lion's share of the correspondence would be that of the famous William Byrd II, about whom we already know a great deal. Fortunately, Tinling's sleuthing has uncovered many epistles of the first and third William Byrds. (The breakdown in these volumes is as follows: William Byrd I, pp. 8–191; William Byrd II, pp. 203–600; William Byrd III, pp. 615–821.)

What will these letters do to alter or enlarge our image of the Byrds? Certainly they will not drastically change one's perceptions of William Byrd II, save for matters of small detail. The same observation is even true in large part for the first Byrd if one has read Pierre Marambaud's informative articles on the founder of this American family that appeared in the *Virginia Magazine of History and Biography* in 1973 and 1974. If the progenitor of the Byrd fortune was shrewd, hardworking, and versatile as an entrepreneur, he also shared some of the intellectual interests that his son is remembered for.

It is William Byrd III, however, who has been neglected and who emerges in a somewhat better light, a man whose propensities for extravagance and careless business management led him down the road to financial ruin and suicide. Even so, his missives indicate that there was much of an admirable nature about him. Like his father, he took his public responsibilities seriously. He commanded a Virginia regiment in the French and Indian War. And as Tinling observes: "He had endured hardships and shown great concern for his men. He had expended huge sums of his own fortune. He had lived away from his family for five years, while his mother ran Westover and his friends tried to keep his affairs in order at home" (p. 609). He also served in the House of Burgesses and occupied a seat on the Council for two decades. It should be

recalled that he was not the only member of the planter elite to fall upon adversity in the 1760s and afterward. So did George Washington, another French and Indian War veteran. Luckily for the nation-to-be, the squire of Mount Vernon turned his circumstances around; William Byrd III was not so fortunate.

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JAY HIGGINBOTHAM. *Old Mobile: Fort Louis de la Louisiane, 1702–1711*. (Museum Publication, number 4.) Mobile, Ala.: Museum of the City of Mobile. 1977. Pp. xiv, 585. \$25.00.

On November 2, 1897, in Mobile, Alabama, Peter J. Hamilton signed the preface to his *Colonial Mobile* (1898); over three-quarters of a century later, on November 11, 1975, also in Mobile, Jay Higginbotham signed the two-page preface to his *Old Mobile: Fort Louis de la Louisiane, 1702–1711*. The *Colonial Mobile* of Peter Hamilton, which in its day was considered a historical and literary classic, has at last found a worthy historical companion.

Jay Higginbotham classifies his book as "for the most part, a local history: an attempt to describe in as detailed and accurate a fashion as is presently possible the personalities and events surrounding the establishment and life of the now extinct town known to history as Old Mobile." And he does just that—using a total of 457 pages of text, a 58-page section of illustrations and maps, a series of documentary appendixes, and 14 pages of archival sources scattered among depositories in Europe and the Americas. Thus, the work has breadth and depth seldom found in a local history study.

There are twenty-two chronologically arranged chapters, each centered around important individuals, events, diasters, crises, plagues, or other subjects. The heroes and villains of those early Mobile years are all present; Iberville and Bienville, Tonti of the Iron Hand, diligent and not-so-diligent priests and government officials, Indians savage and gentle, red and black slaves, the Casket Girls of Mobile—who arrived at their settlement more than two decades ahead of the Casket Girls of New Orleans—and numbers of ordinary common folk of the fort and village all pass in review.

The author has covered his self-assigned area and time period with notable distinction. Readers of southern colonial history will acquire a wealth of new information and an appreciation of the rigors of French and Spanish pioneer life. Some of them will find new evidence that French Louisiana was or was not "a step-child of Mother France." A few may decide that Iberville and Bienville, with

all their weaknesses of ordinary men, deserve their places of eminence in Louisiana history. Historiographically, it is obvious that the author of this volume has now become a member of that small group of historians who have made really significant contributions to the colonial history of the entire central Gulf coastal region.

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ALAN TULLY. *William Penn's Legacy: Politics and Social Structure in Provincial Pennsylvania, 1726-1755*. (Johns Hopkins University Studies in Historical and Political Science, number 2.) Baltimore: Johns Hopkins University Press. 1977. Pp. xvi, 255. \$14.00.

The most important thing about this book is the question it asks; the question is more important than the research or the answer to the question. How did Pennsylvania achieve political stability in the second quarter of the eighteenth century? Alan Tully expertly narrates the story of politics in Pennsylvania between 1726 and 1755; he has accumulated everything that we can reasonably expect to know about individual assemblymen for that period—religion, wealth, length of service, committee assignments, and geographic distribution; and he has woven it all into a clear interpretive structure. But all of this is subordinated to the single query about stability.

Tully takes his bearings from two books: J. H. Plumb's *Origins of Political Stability: England, 1675-1725* (1967), which he emulates, and Bernard Bailyn's *Origins of American Politics* (1968), which he disputes. The central point of the book is that for twenty-five years Pennsylvania politics were not contentious, factious, or turbulent, as *The Origins of American Politics* characterized all eighteenth-century colonial governments. Pennsylvania entered on calm seas at about the same time as Robert Walpole stabilized the English ministry. Tully sets out to explain Pennsylvania's stability as Plumb uncovered the reasons for Walpole's long preeminence in Britain. At the end of the book, Tully points to other colonies with similar peaceful stretches and implicitly raises the question, did stability visit large portions of the empire in the mid-eighteenth century?

The key word in the analysis is stability. Conflict did not disappear after 1726. The Penns' tough quit-rent policy in the 1730s and Governor George Thomas's heavy-handed mobilization of military forces for King George's War drove a wedge between the Quaker-led popular party and proprietary leadership. By the late 1740s only some of the Philadelphia merchants and a few Bucks County

families consistently supported the government. In the same years hordes of German and Scots-Irish immigrants created diverse cultural and ethnic enclaves in the rapidly expanding colony. In the 1740s the Great Awakening split the Presbyterians and shook up other religious groups.

The point Tully stresses is the capacity of Pennsylvania to absorb these shocks without losing its balance. The colony's political, economic, social, and ecclesiastical systems proved their resilience and strength. As heirs of William Penn, Pennsylvanians prized peace in politics and tolerance in religion. The social and political orders were sufficiently supple to enable the colony to restore the desired harmony after each period of turbulence. The bulk of the book is devoted to an explication of the complex combination of elements that permitted Pennsylvania thus to honor William Penn's legacy.

Is the analysis persuasive? Yes and no. Yes, because most everything Tully says is plausible. No, because by the end of the book it is evident that the question need not be answered. Merely asking it suffices. Raising the question of stability, when fragility and instability were assumed to be the dominant qualities of colonial politics, immediately exposes the vagueness of the categories of analysis. There is nothing substantial in them to be explained. Stability and instability suffer from the same deficiency as conflict and consensus. Their reality is in the eye of the beholder. Pennsylvania was sometimes turbulent and sometimes peaceful between 1726 and 1755. The proportions of harmony and discord differed in comparison to other periods and places but not so sharply as to require a distinctive explanation.

In his illuminating analysis of stability in England, J. H. Plumb had the advantage of working toward one indisputable fact: Robert Walpole remained the king's first minister for an unusually long time. The opposition did not wither away between 1721 and 1742, nor did society lapse into quietude. But through it all Walpole stayed in power. That fact had to be dealt with.

Politics in Pennsylvania offered no such simple index of stability. In his concluding pages, Tully enlarges on the complicated and difficult inquiry that must be mounted to determine whether a political regime was stable or not, or, in other words, whether there is anything to be explained. The subtlety, the intricacy, and the obscurity of this inquiry suggests the prudence of formulating new categories of analysis. Thanks to the inadvertent perspicacity of Tully's question, the study of colonial politics can move on to the explication of new themes.

RICHARD L. BUSHMAN
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GREGORY A. STIVERSON. *Poverty in a Land of Plenty: Tenancy in Eighteenth-Century Maryland*. (Maryland Bicentennial Studies.) Baltimore: Johns Hopkins University Press. 1977. Pp. xv, 187. \$12.00.

For many years tenancy in colonial America remained a mystery to historians who have ignored the institution because its records seemed unavailable and, perhaps more importantly, because they assumed that the yeomanry was the mainstay of agricultural development and tenancy only a short-term aberration. Recent publications, however, have proven historians wrong on both counts. This book by Gregory A. Stiverson, Assistant Archivist at the Maryland Hall of Records, will surely stimulate our growing awareness of the importance of this neglected subject.

Focusing on eight sample manors (comprising about 48,000 acres and 300 tenants) of Lord Baltimore, Stiverson's work presents many fascinating details of eighteenth-century Maryland tenancy. His approach is basically quantitative, and the book is sprinkled with tables. It gives a clear picture of tenant life, which was dominated by abject poverty.

Reflecting this poverty were the meager personal holdings of the leaseholders at their death, the absence of slaves or servants working for tenants, the small tenant dwellings, the inconsiderable improvements on their land, and their inability or unwillingness to purchase and retain the reversion (soil right) to their leaseholds even when they had the opportunity.

Stiverson argues that the tenants were given ample amounts of land (140½ acres on the average), generous lease tenures, and low rents (slightly higher than the quitrents charged on freehold land). Consequently, Stiverson finds the tenants "barely distinguishable from small freeholders." Nevertheless, most of them never accumulated enough capital to buy a freehold and were thus condemned to poverty forever.

One probable cause was poor soil quality, either naturally poor or depleted. But the net result was poor crop yields. It appears from the evidence presented here that the tenants could have overcome the problems if they had had the use of slaves and servants; a labor shortage was the principal impediment to their economic advancement. Still another problem was the large number of tenant children who had to be fed. Tenants on the Maryland Eastern Shore and upper Western Shore manors, especially those without slaves, improved their lot by shifting from tobacco to wheat and more diversified farming. But other tenants were unable to do so for psychological and economic reasons. Despite their poverty, Stiverson emphasizes, the leaseholders held tenaciously to their

land simply because they lacked the means, or even possibly the aptitude, to change. Thus, the emerging tenant society was stagnant, offering neither economic nor geographical mobility for its members. Above all, it offered them no hope for a better life.

Stiverson's theses are provocative and surely deserve careful attention. But I am bothered by some of his internal contradictions and his tendency toward sweeping generalizations without adequate evidence. For example, he argues that poverty was endemic among manorial leaseholders. Yet his own evidence shows that 30 percent of original leaseholders and 38 percent of tenants owned freehold lands, and that none of them sold his manor lot after becoming a freeholder. Such people would have disposed of their leases, embryonic capital, if they had found them to be unprofitable. Stiverson also shows that some merchants invested in manorial leaseholds for speculative purposes. These and many other examples cited in the book demonstrate that Maryland proprietary tenancy was heterogeneous in class composition, relatively mobile, and not uniformly poverty stricken. Finally, Stiverson tells us that his study of very fragmentary inventories of estates and tenant dwellings shows that they had little or nothing to bequeath upon their death. Yet it is also clear that their improvements were rated 10 to 20 shillings per acre, and leases with improvements were frequently bought by other tenants. Unfortunately land improvements escaped thorough investigation by the author. This is especially deplorable given the unreliability of the will and inventory of a testator's estate as a measure of his economic worth during life. One also wonders about Stiverson's bias when he characterizes tenant dwellings measuring about sixteen feet by twenty-four feet as "little better than hovels" and "an uninviting blemish on the landscape." I doubt if poor, recent immigrants with large families, institutional paupers, present or former indentured servants, and even tenants under private landlords, whose conditions were allegedly much worse than manorial tenants, would have agreed with Stiverson's characterization.

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JOHN PHILLIP REID. *In a Defiant Stance: The Conditions of Law in Massachusetts Bay, the Irish Comparison, and the Coming of the American Revolution*. University Park: Pennsylvania State University Press. 1977. Pp. 225. \$14.50.

Comparative history should light places single-focus lamps leave dark; this book does that well.

John Phillip Reid indicates in his subtitle what he will compare, but he cannot there hint how richly such comparisons (including also English/British prototypes) inform us of conditions of law in dependent societies. He describes control of law in times of turbulence and how men who controlled imperial and local law used those instruments in their labors. Despite surface parallels between American and Irish circumstances, those colonies expressed their self-interest within the context of British control in markedly different manner. In America, moderation accompanied revolutionary rhetoric; in Ireland, threats to public safety activated ready gallows. "The British legal legacy to North America was exemplified by the whigs of Boston who sought to establish the legitimacy of their actions while throwing East India Company tea into the harbor. The British legal legacy to Ireland is exemplified in the death of Lord Kilwarden . . . dragged from his coach in the streets of Dublin and hacked to death by rebels bearing pikes" (p. 156).

This we might have known or imagined that we knew; Reid now erases any ambiguity by carefully explaining the connections and mutual reinforcements between crimes and punishments and between lawful and lawless extensions of "the law." He describes the harvests users of the law anticipated and achieved. When the lesson ends, the reader has been taught that "what was different [between Dublin and Boston] was not the substance of the law but the tradition of law and the degree of local control over the institutions of law" (p. 173)—especially the instruments of juries (grand and traverse, both civil and criminal) and the ways they were employed or ignored in political confrontations. He has been guided through evidence illustrating how "control of the judiciary in a political struggle may well determine which side is punished and which is protected" (p. 3), the differences in perception that allowed Whigs to see law where Tories saw anarchy, and why it was that "in Rhode Island, if not all of pre-revolutionary North America, when the mob was not running through the streets it was sitting in the jury box" (p. 86).

We know then what the law was and how it worked or, in the author's phrase, "the condition of law." We are less informed about why law worked the way it did or what ingredients led Massachusetts Whigs to find and use British law one way and Irish subjects another. Some day, perhaps, Reid will extend his inquiry from this excellent base in its next most obvious direction.

CARL UBBELOHDE

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RICHARD ALAN RYERSON. *The Revolution Is Now Begun: The Radical Committees of Philadelphia, 1765–*

1776. Philadelphia: University of Pennsylvania Press. 1978. Pp. xv, 305. \$25.00.

The most conspicuous feature of scholarship on the American Revolution during the past twenty-five years has been sustained concentration on the political perceptions, beliefs, and values of the men who led colonial resistance to British authority during the 1760s and 1770s. For the most part, however, historians have not sought to explicate the processes through which strategic segments of the general population came to share fundamental concerns of Whig ideologues and were mobilized for revolutionary action. Richard Alan Ryerson's aim is to help fill this void with respect to colonial America's foremost city. Through intensive study of the formation and operation of the Whig committees that were virtually the sole organizations for resistance activity in Philadelphia throughout the imperial controversy, Ryerson seeks to disclose how the ideology of the Revolution was carried "one last step, out of the realm of theory and rhetoric into the domain of reality and action" (p. 5).

The central theme of Ryerson's book turns on how Philadelphia's Whig leadership maintained and intensified the momentum of anti-British activity by gradually and skillfully broadening the composition of the resistance movement to incorporate an ever widening proportion of the city's diverse economic, social, religious, ethnic, and occupational groups. This recruitment process was so successful that by the time the Continental Congress began its sessions in Philadelphia late in the summer of 1774, the extralegal committee system had become a dominant force in Pennsylvania politics. In addition, it had spurred the emergence of a new political elite—economically, ethnically, religiously, and vocationally more diverse than the established Quaker-merchant oligarchy that had long dominated politics in the city. Between 1774 and 1776, however, this elite was itself challenged and supplanted by an even newer, less affluent, and more pluralistic group of leaders from Philadelphia's large and aggressive middle class. By the spring of 1776, this group—composed almost exclusively of "new men" who had acquired their first taste of political power in the resistance movement—successfully led the campaign for independence, engineered the overthrow of the Pennsylvania Assembly, and created a radically democratic constitution for Pennsylvania.

Ryerson thus confirms the familiar Progressive interpretation of the Revolution as a dual movement centering ultimately as much upon who would rule at home as upon home rule. At the same time, he gives that interpretation, as applied to Pennsylvania, a richness of detail and subtlety of treatment it has heretofore lacked. His careful

quantitative analysis of the changing composition of Philadelphia's Whig committees, combined with his close examination of the machinations of Whig leaders from 1773 to 1776, yields a penetrating view of the dynamics of resistance politics during those years.

If there are serious shortcomings to Ryerson's work, they are ones of exclusion rather than of execution. The process of mobilizing colonists for political action was dependent upon a corresponding process of persuading them that British policy seriously endangered their most vital rights and interests. Yet Ryerson gives but scant attention to the rhetorical means used by Whig leaders to energize public support and to recruit committee members. In addition, although Ryerson assiduously identifies and classifies Philadelphia's Whig committeemen, he does not seek to explain why certain members of the community became active participants in the resistance movement while others did not. Attending to such matters would have allowed Ryerson to bridge more fully the interstice between ideology and action but would also have altered substantially the character of his book. As it is, *The Revolution Is Now Begun* is an instructive work that requires the close attention of all students of Pennsylvania politics during the British-American conflict.

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MARK A. NOLL. *Christians in the American Revolution*. Grand Rapids, Mich.: Christian University Press. 1977. Pp. 195. Paper \$4.95.

This volume contributes admirably to the current broadside of scholarly comment upon the American Revolution. Succinctly and clearly, Mark A. Noll synthesizes much recent research tracing attitudes of Christians to the Revolution. At mid-eighteenth century, the dominant religious pattern was "Puritanism," with its holistic orientation to the community, its millennial thrust, its moralism, and its dissenters' concern for "liberty." Reinvigorated—and somewhat modified—by the Great Awakening, Puritanism blended readily with the "radical Whiggism" described by Bernard Bailyn to produce a religiously suffused revolutionary atmosphere and ideology.

Noll's main contribution is to provide convenient summaries of four major patterns of religious response to the Revolution. The "Patriots," representing "vast numbers of American Christians" (p. 51) and chiefly Congregationalist and Presbyterian, uncritically conflated Christian and radical Whig doctrine. Men like John Witherspoon came to "visualize oppression as the greatest enemy and

liberty as the highest good" (p. 59) and to give unreserved support to the American cause. The "Reformers," a small number of revivalist New Lights, generally supported the cause but refused to identify it with absolute good, thus preserving "the integrity of religious convictions" (p. 102). Isaac Backus, for instance, found the general appeal to "liberty" inconsistent with continuing patterns of religious intolerance. The "Loyalists," among whom northern Anglicans were heavily represented, opposed the war. The "Pacifists," finally, were members of the historic peace churches: Moravians, Quakers, Dunkers, and Mennonites. For them, "questions concerning the morality of warfare itself were more important" than questions about loyalism or patriotism (p. 123).

In two concluding chapters, the author traces lasting effects of the revolutionary experience upon the nation and churches. Most notably, the Revolution appears to have exerted a massive secularizing influence upon American intellectual life. If by mid-century clergy were the most prominent leaders of thought, and theology was the principal form of intellection, the Revolution displaced religion with politics and thrust forward men like Jefferson to set the tone of American thinking.

There are a few tares in the wheat. Noll's account concentrates overwhelmingly upon New England. It neglects important background in the profound earlier reorientation of Puritanism which Timothy Breen has found coming to focus in the property-conscious Glorious Revolution of 1688. It discusses the impact of the Awakening but not of the Enlightenment upon American religious traditions. It does not comment upon Sidney Mead's thesis that American revolutionary and constitutional doctrine is rooted in a "religion of the Enlightenment" neither synonymous with radical Whiggism nor congenial to Puritanism. It does not explain how self-interested Baptist exploitation of libertarian rhetoric is to be construed as a high-minded exercise in "religious integrity."

On balance, this is a careful and expert study, a convenient summary for scholars and eminently suited for college-level assignment.

THEODORE DWIGHT BOZEMAN
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THOMAS E. BUCKLEY. *Church and State in Revolutionary Virginia, 1776-1787*. Charlottesville: University Press of Virginia. 1977. Pp. xii, 217. \$12.50.

This monograph by Thomas E. Buckley is of modest proportions. Its purpose is to look at Revolutionary Virginia and the struggle over the question of religious liberty which resulted in the monumental act "for Establishing Religious Freedom"

of 1786. The text focuses on the years between 1776 and 1787, during which the Virginia Assembly abandoned support for the concept of a state church and took the position that the state would have no right of interference with an individual's religious conscience. In so doing, the author concludes, Virginians were ahead of their times; they were unique; they had selected a "genuinely revolutionary course of action" (p. 173), which served as the guiding model for the other rebellious states in the months and years ahead.

The text thus is concerned with the political struggle, primarily in the Virginia Assembly, over whether religious freedom and separation of church and state could become legitimate concepts. Too often, argues Buckley, historians have extolled the contribution of the rationalists to the debate while ignoring the evangelicals, as if the latter group were incapable of more than fervent emotions. What the author demonstrates is that a kaleidoscope of opinions prevailed and that the evangelicals (based principally upon their numerous petitions) tended to see the advantages of complete separation more clearly than did many rationalists. Indeed, some rationalists continually stressed the importance of some form of religious establishment, even if it meant state-endorsed taxation in support of all fully institutionalized churches, so that the churches would have the funds to serve as morally stabilizing bodies by encouraging virtuous behavior among the republican citizenry. If anything, the evangelicals, having been needlessly stifled more than once by the Anglican Church, saw the advantages of religious liberty in as pure a form as did Thomas Jefferson, and they thus generally avoided the republican morality position as inhibiting freedom of conscience.

While Buckley's arguments have many merits and serve to refine our knowledge about the search for religious liberty during the American Revolution, the text is exceedingly difficult to follow. Denominational groups, petitions, and draft bills come and go so fast that the effect often seems to be that of a hopelessly tangled collision of random forces. Quite often, the reader has no idea where the text is leading or, more properly, what is of significance. To take a specific example, there are four maps diagramming such patterns as the 1784 Assembly votes on religious bills by county (p. 110). But the possible importance of the visualized material in this and the other charts is hardly touched upon. Readers will naturally ask why the author included the maps when their possible meaning has been largely ignored. This example points to the broader textual problem of descriptive cluttering, with only occasional attempts to analyze the evidence presented.

Despite its vagaries, specialists should tackle this volume. The arguments over religious toleration in Virginia reveal a great deal about the struggle to define the essence of a republican order in Revolutionary America. In looking at that struggle, Buckley has made a contribution to historical literature on ideology, religious values, and revolutions.

JAMES KIRBY MARTIN
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HAROLD B. HANCOCK. *The Loyalists of Revolutionary Delaware*. Newark: University of Delaware Press. 1977. Pp. 159. \$11.50.

Harold B. Hancock published a pioneering monograph, *The Delaware Loyalists*, in 1940, reissued in 1972 by the Gregg Press in its estimable series, "The Loyalist Library." Now Hancock has expanded and somewhat changed his original "youthful effort" (p. vii) in the light of recent scholarship and his own further research. He appears to have consulted everything relevant to Delaware, primary and secondary. He has added a map (difficult to read), four illustrations, and a bibliography. He has retained an interesting appendix which lists the names of "Loyalists Ex-cepted from Pardon by the Act of June 26, 1778," together with expanded, but still brief and spotty, information about them. This time he has also printed the act itself.

The new book is better than the old. As Hancock remarks, earlier he ignored the very important conservatives and moderates, seeing people "in terms of black and white" (p. viii). (However, his sympathies still seem to be completely with the Revolution.) Aware of the difficulty of definition, he states the proportions of patriots and loyalists much less precisely. In keeping with modern trends there is less emphasis on Thomas Robinson, Delaware's leading loyalist, and more on the rank and file. Thus, he handles motivation more subtly and pays more attention to the characteristics of each of the three lower counties. Religion remains important, but he adds more on such things as economics and family relationships.

A strength of the book is its broad chronology—1765 to the early 1800s. Unlike some scholars, Hancock is aware that "the division of Whig and Tory continued long after" 1783 (p. 109), and he notes support of former loyalists for the Federalist Party. Another strength is an attempt at comparative history: for example, the Black Camp rebellion is likened to Shays's, and Delaware loyalists are shown to be similar to those of Maryland's eastern shore.

There are some slips and oddities (Lewis Namier becomes Louis, the PRO becomes the British

Record Office), some repetition, some problems with the narrative, and some elements of anti-quarianism. This is, however, a useful addition to local studies, one of the most fruitful approaches to the varied phenomenon of loyalism.

WALLACE BROWN
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ALLAN S. EVEREST. *Moses Hazen and the Canadian Refugees in the American Revolution*. (A New York State Study.) Syracuse, N.Y.: Syracuse University Press. 1976. Pp. xi, 217. \$12.95.

The Bicentennial celebration brought, among other things, new recognition to a number of neglected heroes of the Revolution. Moses Hazen, a major landholder in America and Canada prior to the war, was a unique individual. After being imprisoned by both sides and offered a commission in the Continental and British armies, he cast his lot with the Americans and commanded a regiment of Canadian-Americans. His story writ large was that of the colonists living in Canada who came south to aid the Americans in their struggle for independence.

America's fight for a fourteenth colony proved futile. Whenever invading armies retreated, the neutral or pro-American sentiment in the French-Canadian countryside diminished. Hazen was forever pestering Washington to mount yet another offensive in Canada, but the Anglo-American Protestant minority succeeded in keeping the colony within the empire.

Hazen had a distinguished career. During the French and Indian War, he fought at Crown Point, Louisburg, Quebec, and Sillery, rising to company commander and winning a commendation from General Wolfe. Commissioned a lieutenant in the British army, he retired on half pay. In the Revolution, his regiment of Canadians called "Hazen's Infernals" fought at Long Island, Brandywine, and Germantown. After making himself useful to the Americans in their Canadian invasion in 1775, Hazen advocated further operations and helped plan the abortive Canadian offensive in 1778. When this misguided venture was abandoned, he proposed a military road be built to the Canadian border and worked on the project in 1779. Brevetted a brigadier general in 1781, he commanded a brigade in Lafayette's division just before the allied armies closed in on Cornwallis at Yorktown.

This biography is a singularly undistinguished book. A traditional, old-fashioned work written mainly from official sources, it presents the details of Hazen's life with accuracy. But there is little or no interpretation, and we get no glimpse of the

man of flesh and blood. Hazen was a man obsessed. Any man who slaughtered women and children in warfare, seduced his neighbor's wife, and fought as much, if not more, with his fellow officers as the enemy, deserves greater analysis. Although there is a lame attempt on the last page at psychohistory through a psychologist's quotation on Hazen's life, nothing more is made of the matter. For a biography of Hazen and for the subtleties involved in trying to pay dual loyalties to two countries in wartime, we must await a more thoughtful work.

GEORGE ATHAN BILLIAS
Clark University

HENRY J. BOURGUIGNON. *The First Federal Court: The Federal Appellate Prize Court of the American Revolution, 1775-1787*. (Memoirs of the American Philosophical Society, number 122.) Philadelphia: The Society. 1977. Pp. 362. \$10.00.

In this volume, Henry J. Bourguignon supplies a comprehensive examination of the predecessor institution of our federal courts, deriving in the process important insights into the motivations and understandings of the Constitution's framers.

In Part I the author surveys Congress's response to the need for prize courts, identified by George Washington as early as November 1775. Congress authorized the capture by duly commissioned privateers of vessels engaged in war against the United Colonies, of cargoes for the British army, and of vessels carrying such supplies. Instead of establishing a federal court to decide captures, Congress recommended that the states establish their own courts, with trial by jury—a departure from British practice. Congress did, however, reserve to itself the power to issue commissions to privateers, adopt uniform jurisdictional rules for the state courts, and provide for appeal in all cases to Congress or to persons designated by Congress. Litigation over prizes commenced in early 1776, with Congress acting as a trial court, resolving petitions filed from states that had not yet established admiralty courts. On July 4, 1776, as Congress issued its famed Declaration, the losing party in a Pennsylvania prize court proceeding requested the first "federal" appeal.

Initially, Congress appointed ad hoc committees to hear individual appeals. Then, on January 8, 1777, Congress appointed a standing Committee on Appeals; during May 1777, the Committee was authorized to appoint a register, its first professional staff. In February 1780, the appeal function was assigned to the Court of Appeals in Case of Capture, staffed entirely by nonelected professionals. Analogous processes led Congress eventually

to appoint professionals to run activities such as finance and foreign affairs, ultimately assigned by the Constitution to the executive branch.

The major problems Congress encountered in connection with prize appeals were also similar to those it encountered in other areas. Some states insisted on controlling the right to appeal to Congress. Others insisted that the findings of juries could not be reviewed, a highly significant position, since local juries were extremely biased in favor of local citizens and insensitive to foreign affairs considerations. Finally, just as Congress lacked power to enforce its decisions concerning commerce and its requisitions, it lacked power to enforce the decisions of its Committee on Appeals. This became clear in the case of the sloop *Active*, when Pennsylvania refused to enforce an order of the Committee on Appeals despite Congress's express resolution that the Committee's order be enforced.

In addition to this background, Bourguignon provides a detailed description of procedural and substantive prize law in late eighteenth-century America. Though this material might have been drastically edited with little loss in meaningful coverage, its availability to scholars in this single volume is a powerful argument for inclusion. Far less can be said, however, for the author's exhaustive and exhausting discussions of British prize law (particularly an unfortunately dull, thirty-six page introduction which may well prevent some readers from reaching the lively material that follows). The British material adds very little beyond rather straightforward lessons, such as the facts that the British never used juries in prize cases and that British procedures were more formal than those adopted by the states.

The book ends with two brief chapters summing up Congress's experiences in prize cases and weighing the influence of these experiences on the Constitution. Direct evidence of impact is meager, as Bourguignon carefully notes. He does an impressive job, however, marshalling the indirect evidence. Among the individuals intimately familiar with early prize-court experience were James Wilson, Oliver Ellsworth, Edmund Randolph, and James Madison; some twenty-one of the fifty-five members of the Convention of 1787 had been directly involved in the work of the Committee on Appeals or the Court of Appeals, either as judges or as counsel. Several of the Convention's decisions appear related to prize-case experience: to create a separate and independent judicial branch; to vest disputes likely to create international controversy in federal courts; to authorize Congress to establish lower federal tribunals instead of mandating a system of state trial courts with review in a federal Supreme Court; to refuse to provide for jury trials

in all cases (a matter resolved by the Seventh Amendment's guarantee of jury trial in "Suits at common law," thereby excluding admiralty cases); to grant the Supreme Court expressly the power to review "fact" as well as law; and to make federal law supreme in its sphere over state decisions. This last conclusion received concrete validation in *United States v. Peters* (1809), when Chief Justice John Marshall upheld federal authority over the disposition of the sloop *Active*, the very dispute that had demonstrated the Continental Congress's incapacity.

ABRAHAM D. SOFAER
Columbia University

E. JAMES FERGUSON and JOHN CATANZARITI, editors. *The Papers of Robert Morris, 1781-1784*. Volume 3, *October 1, 1781-January 10, 1782*. Pittsburgh: University of Pittsburgh Press. 1977. Pp. xxxvi, 557. \$22.50.

When Robert Morris dealt with problems of federal finance, the public purse was close to empty, but the demands on it were simple. During the three months covered in the third volume of his papers, Morris stalled the nation's creditors, hounded the states for funds, and arranged to pay for military supplies. Such matters were certainly difficult enough to deal with, but at least Morris was not troubled with requests to fund volumes of documentary history.

Today the federal government, through the National Endowment for the Humanities, pays most of the bills for the Morris Papers project. This circumstance doubtless explains the substantial editorial changes that have been made in this volume. New annotation policies are described in a terse note sandwiched between the list of location symbols and the list of short titles, but no attempt is made to justify them. Henceforth, the editors announce, annotation will be "restricted to subjects essentially related to Morris's conduct of the Office of Finance" (p. xxix). For more obscure topics, where the reader is most likely to need help, annotation is severely abridged or omitted altogether. Cross references have been cut down as well. Since the annotation of the Morris Papers was always lean, these changes are not a matter of trimming fat; they cut into muscle and sinew.

Considering the new policy strictly as an editorial decision, it would appear to be indefensible. Although volume 3 of *The Papers of Robert Morris* is still a valuable research tool, it is decidedly inferior to the previous volumes. But considering the situation realistically, some change from the earlier practices of this editorial project was probably unavoidable. At a time when federal funds have many claims made on them—by the poor, the ill,

and the elderly (not to mention the illiterate)—a full-dress letterpress edition running to a dozen volumes to cover three years in the public life of one individual is hard to justify. Nevertheless, it may be asked whether pruning the editorial apparatus is the best way to economize.

There are alternatives. Printing only select documents is one. Publishing in whole or in part in microform is another. Since reassembling the Morris archive was a major achievement in itself, failing to make the entire collection available would be deplorable. Cutting back here seems as unappealing as cutting back the annotation. The third alternative is not exactly attractive. Where is the scholar whose eyes are so resistant to strain that he will give up the luxury of bound volumes as handsome as these without a sigh? Would that money grew on trees and sacrifices for the sake of economy were not necessary! But money does not grow on trees, as Robert Morris, who died a bankrupt, would himself have been forced to admit. And if sacrifices must be made, luxuries should be given up ahead of essentials.

LINDA GRANT DE PAUW
George Washington University

JEFFREY J. CROW. *The Black Experience in Revolutionary North Carolina*. (North Carolina Bicentennial Pamphlet Series, number 16.) Raleigh, N.C.: Department of Cultural Resources. 1977. Pp. ix, 121. \$1.50.

Slave resistance is the central theme of Jeffrey J. Crow's history of blacks in eighteenth-century North Carolina. Crow rightly argues that blacks were not passive recipients of white culture and order; rather, blacks were calculating and alert men and women who used religion and African culture to save themselves from humiliation and degradation. Despite differences among themselves—differences between slave and free, urban and rural, church and unchurched, native-born and African-born—blacks in North Carolina forged a separate Afro-American community by the end of the eighteenth century. They appropriated evangelical Christian themes of apocalypse and the self-worth of all men and mixed them with African religious practices to create a religion of hope and resistance. Blacks struggled to maintain the integrity of their families, and they preserved their African culture through crafts and the skills of rice cultivation, which ironically made them more valuable to their masters while making them more dangerous as well. Crow's portrait of blacks in North Carolina borrows heavily from the work of John Blassingame, Eugene Genovese, Herbert Gutman, Gerald Mullin, and Peter Wood. Crow adds significant shadings, but he gives us a com-

posite drawing. Given the medium, it is a work of rich texture and subtlety.

Still, a curious imbalance mars Crow's book. Crow devotes almost half of his narrative to descriptions of slave resistance, particularly overt acts such as running away, plotting or engaging in rebellions, or dissembling. Even his sections on the North Carolina slave code and the economics of slavery assume that resistance was the primary fact of black life in the eighteenth century. Perhaps it was. The eighteenth century favored resistance. The institution was not fully shackled on blacks and the South, and political controversy and the ideology of the American Revolution threatened it. Finally, the dislocations of the Revolutionary War allowed many blacks to escape to freedom. The Revolution came at a time when native-born blacks, particularly artisans, were becoming increasingly assertive and self-confident in their Afro-American community. The result was inevitable—slave insurrection. Crow concludes his book with a detailed and insightful analysis of the slave "uprising" of 1802. By so doing, he suggests that resistance was the principal legacy of the eighteenth century. Yes, partly. But the emphasis on resistance obscures another trend. However much blacks sought cultural autonomy and freedom, they did not achieve them. In North Carolina anyway they were isolated and outmanned, and the whites were not blind or stupid. For Crow, the planters appear as one-dimensional men gripped by fear and incapable of understanding the blacks who terrified them. In fact, the planters recognized the humanity of their slaves; they assumed that as men the slaves would rebel against bondage and injustice. The planters wrote slave codes to tighten the system of control, but they also sought to "ameliorate" some of the physical conditions of slavery to "win over" the slaves. Slaves lived as much by adjusting to this circumstance, by working in the interstices of control, as through overt resistance which was doomed to failure. Not all slaves were capable of the heroic, and indeed survival may have been the most heroic defiance of all.

These criticisms, which really reflect differences in interpretation, should not detract from Crow's significant achievement. He has written an excellent account of slavery in its formative period in North Carolina; he has synthesized an immense literature on slavery and made it intelligible to the average literate reader; and he has provided a fresh perspective from which to assess blacks and slavery in America. That he has done so in a book of approximately one hundred pages is a testimony to his dexterity and his command of the subject.

RANDALL M. MILLER
*Saint Joseph's College,
Philadelphia*

EDWARD C. CARTER II, editor. *The Virginia Journals of Benjamin Henry Latrobe, 1795-1798*. In two volumes. New Haven: Yale University Press, for the Maryland Historical Society. 1977. Pp. lxxx, 293; xii, 297-575. \$60.00 the set.

Among the several projects supported by the National Historical Publications and Records Commission, the papers of Benjamin Henry Latrobe are noteworthy for their many-sided cultural significance and for their importance in the history of American art. Following close upon a massive microfiche edition of Latrobe's papers, the *Virginia Journals* are the first two published volumes of a projected ten, which will cover the extant journals, selected correspondence, watercolors, and architectural and engineering drawings of this highly talented and seminal figure. The present volumes in substance and format fittingly contribute to an impressive memorial for the first professionally trained architect and the first major steam engineer in the United States.

The son of a leading Irish-born Moravian minister and a Pennsylvania mother, Latrobe was born in 1764 in Yorkshire and given an excellent classical education in Silesia and Saxony. Subsequently in England he trained in engineering under John Smeaton and in architecture under Samuel Pepys Cockerell. He set up his own architectural practice, but after the death of his wife he emigrated to America in 1795 to commence a new career, soon undertaking the Virginia State Penitentiary (1797-98) at Richmond, the Bank of Pennsylvania (1798-1801), and the Waterworks (1799-1801) in Philadelphia, which helped initiate the Roman and Greek revivals in American architecture. He designed in Sedgeley (1799) the first Gothic revival house in the United States. The Baltimore Roman Catholic Cathedral (1804-18) in a neoclassical style was his finest work. Jefferson appointed him surveyor of the public buildings in 1803, and he served as architect of the Capitol (1803-12, 1815-17), where his famous American orders are still pointed out. As an engineer for the Navy, he supervised construction of the Washington Navy Yard and directed several river, canal, and waterworks projects. As a teacher Latrobe had a great impact on the first generation of American-born engineers and on such important architectural pupils as Robert Mills and William Strickland. He died in 1820 while supervising the New Orleans waterworks.

The present two volumes include ten journals compiled between 1795 and 1798, the first an engrossing account of Latrobe's direful fifteen-week transatlantic voyage. Supplementing the journals are selections from his correspondence and his sketchbooks, along with his *Essay on Landscape*.

Twenty-six color plates and sixty-three figures from his sketchbooks, along with two maps, illustrate the volumes. The editors have also provided a genealogy, a chronology, and a highly useful biographical appendix on lesser-known figures mentioned in the text. The extensive introduction includes a biographical sketch, a discussion of the history and plan of Latrobe's papers, a critical analysis of the *Virginia Journals*, and information on the editorial procedures. The editors' approach to their subject is informative and their estimates are judicious; the editorial apparatus, although somewhat overelaborate in the several exhaustive "editorial notes," is generally exemplary. The reader is easily led to needed background information concerning facts and problems in the text.

Latrobe was a "universal philosopher," well informed, yet curious about and interested in almost all aspects of natural history, philosophy, and manners and customs. The range of subjects in the journals is enormous: flora and fauna, geography, geology, agriculture, herpetology, ichthyology, entomology, meteorology, medicine, law, politics, architecture, education, literature, music, and art. He had a shrewd eye for precise detail in the natural world and the frequently crude society he joined in America and an acute ear for speech patterns and dialects. The journal pages are enlivened with perceptive and amusing observations of unusual "characters" and with the recital of lively anecdotes. The publication of the *Virginia Journals* provides general access to an important source of materials relating to many aspects of life in the South in the early republic.

PAUL R. BAKER
New York University

DONALD M. SCOTT. *From Office to Profession: The New England Ministry, 1750-1850*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xv, 199. \$12.00.

Three insightful books on the professional development of the New England ministry, focusing especially on the role of the clerical office within its contemporary culture and society, have been published in the 1970s: David Hall's *The Faithful Shepherd* (1973), J. William T. Youngs, Jr.'s *God's Messengers* (1976), and now Donald M. Scott's *From Office to Profession*. Consciously borrowing from the work of Hall and Youngs, as well as from the many town studies that have appeared since 1960, Scott posits a colonial backdrop of communalism against which to measure the early- to mid-nineteenth-century transformation of the Congregational and Presbyterian ministry from a communal office to a translocal, self-conscious occupational profession.

According to Scott, this transformation began with the politics of the 1790s, because with the "rise of systematic electoral competition organized by and around two organized political parties" (p. 18) the clergy no longer represented the whole community but only the Federalists, who defended the colonial ideals of elite rule and the politics of deference. But as the Federalists themselves adopted the ways of electoral politics, the public priorities of the party managers and the clergy became incompatible. Thus ministers increasingly turned from the government to private agencies in order to promote religion in society and maintain clerical guardianship of public morality. The establishment of moral societies, beginning in the 1790s, was the first signal that preachers "had abandoned public office to the electoral culture" and had recognized that the "promotion of moral order and social discipline resided in a special fabric of voluntaristic institutions" (pp. 34-35). The second sign of the shift was revivalism, a new instrument of social order whereby a pious national community could be privately gathered through evangelizing the nation.

Scott finds that by the 1830s a "new and demanding style of public commitment emerged from within evangelicalism as a small number of evangelicals began to insist that slavery was a sin and that the true Christian was bound to work for immediate emancipation" (p. 76). Moreover, by 1835 the clergy were polarized into two warring camps—abolitionists and anti-immediatist evangelicals—a division which also plagued the laity in New England congregations. During the widespread debates over the abolition of slavery from the 1830s to 1850s, local pastors suffered the dire consequences of politicizing the ministry, namely, lost pastorates or at best a reduction in salary and status.

Out of the crisis over abolitionism emerged a new conception of the clerical role, one which "set up boundaries of pastoral duty that essentially removed the major sources of controversy and conflict—social activism, uninspiring theological disquisition, and doctrinal partisanship" (p. 128). From the 1830s onward, this new professionalism slowly transformed the evangelical pastor into a physician of the soul rather than, as in the colonial era, a "watchman on the walls of Zion" (p. 130). Hence Scott concludes: "The pastoral role had become almost exclusively a devotional and confessional one" (p. 153). Such pastoralism, equally operative in any New England town, was trans-local and characterized by a self-conscious professionalism.

Although Scott carefully researched his topics and cogently argues his theses, his assertion that moral societies "first emerged in the early 1790s"

(p. 31) is incorrect. Cotton Mather, for one, established a few of these societies at Boston in the early eighteenth century. Moreover, Scott's portrayal of the colonial era as a time of communal harmony is too pat. Scott himself recognized this methodological problem in a footnote regarding an article by James Schmotter: "Looking at the first half of the eighteenth century from the standpoint of the even more stable late seventeenth century, he emphasizes the comparative disorderliness, while I, looking from the standpoint of the extraordinary rate of pastoral turnover of the early nineteenth century, emphasize the comparative stability of the eighteenth century" (p. 158). Scott could have added that few historians of the late seventeenth century regard that era as a "stable" one. But recognizing the problem does not solve it, and Scott's analysis would have been stronger if he had related his themes to a diverse and ever-changing colonial heritage.

Nonetheless, *From Office to Profession* merits a close reading by American historians. For those interested in the New England ministry, there is much to be learned methodologically from Scott's analysis of the clergy's response to the shifting cultural milieu of the nineteenth century. For those interested in modernization, Scott's book is a logical continuation of the work of Robert A. Gross and Richard L. Bushman, tracing their "Puritan to Yankee" motif as it concerns the ministry's relationship to society up to 1850.

GEORGE SELEMENT

Southwest Missouri State University

DONALD R. ADAMS, JR. *Finance and Enterprise in Early America: A Study of Stephen Girard's Bank, 1812-1831*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xi, 163. \$12.50.

This thin book will do sadly little to alter the reputation of financial history as boring and difficult reading. The author apparently aimed simultaneously at an audience of traditional political historians and specialists in the banking field, but he will satisfy neither group. Donald R. Adams, Jr. promises a fresh analytical approach, yet he fails to deliver except in two short, ending chapters, and much of that material has now been published in a leading business history journal. One half of the text is irritatingly filled with long quotations in stilted language, which Adams often substitutes for genuine explanation or uses to illustrate very routine transactions—for example, the opening of accounts with correspondent banks. In addition, the narrative is disjointed and repetitive, despite the book's short length.

Stephen Girard was already established as one

of Philadelphia's most important merchants when he decided, in 1812, to transfer part of his wealth into the banking field. Functioning as a private commercial banker, Girard was strongly opposed by the city's chartered institutions, which refused to honor his bank notes at their windows. By maintaining high specie reserves and other financial techniques, he was nonetheless able to survive and prosper. The early chapters focus on Girard's role in the dissolution of the First Bank of the United States and on the Treasury's efforts to raise substantial funds from bankers to finance the War of 1812. Girard was an active member of a syndicate of subscribers for the government bond issue of 1813, and the book describes fully these negotiations with the Treasury.

The best chapters concentrate on Girard's banking policies and managerial practices. Among the topics discussed are specie holdings, interbank balances, the type and length of loans, collateral requirements, and secondary reserves. Adams found that Girard expected borrowers to maintain compensating balances, and he insisted on a close correlation between the sizes of deposits and loans. The bank's financial data is occasionally presented in a comparative context, but the significance of many of the analytical ratios is never fully explained. Monetary specialists will appreciate the numerous charts and tables describing the bank's financial condition.

EDWIN J. PERKINS
University of Southern California

DREW GILPIN FAUST. *A Sacred Circle: The Dilemma of the Intellectual in the Old South, 1840-1860*. Baltimore: Johns Hopkins University Press. 1977. Pp. xii, 189. \$11.00.

As Merle Curti wrote recently, a major theme among younger scholars in intellectual history has been a preoccupation with symbols and myths and with rhetoric and appearances, and their vocabulary abounds with such terms as tension, irony, tragedy, search for identity, and alienation. This view is well illustrated in Drew Gilpin Faust's collective biography of a group of five Southern intellectuals who considered themselves "a sacred circle" with a divine mission. In Virginia the professors George Frederick Holmes and Nathaniel Beverley Tucker, along with the agricultural experimenter Edmund Ruffin, and in South Carolina the novelist William Gilmore Simms and the politician James Henry Hammond made up the group. They were "family" to each other. They were close friends, they corresponded frequently and visited each other often, they criticized and encouraged each other's work. Each felt himself

endowed with a genius that made him a stranger in the bald and sterile fields of his world. Alienated, seeking an identity, they turned to romanticism as a way to escape the banality of their time. Even in their homeland they felt themselves condemned to exile.

And yet, because of their "born allotment" of intellect, because they were set apart by biological heritage and elected by God to do great things, they felt it a sacred duty to teach less-favored people. They would apply their minds to the mundane affairs of life. By redefining and broadening the meaning of utility, they hoped to include themselves and their work in it. As they did, their rhetoric and their concerns echoed the beliefs of a reformist and evangelistic ideology often assumed to be exclusively Northern in character. Their efforts showed the essential similarities among the intellectuals in both sections in America.

As a religious mission they would overcome their alienation and win acclaim while correcting the imperfections they decried in their society. They would show the way to perfection. Thus they wrote and spoke upon such matters as sectional economic independence through agricultural knowledge and through diversification into manufacturing, they advocated improvements in education, and they called for political changes that would free the South from demagogues and replace them in office with the spiritually elect. All of them were connected in various ways with reviews and journals of criticism which they intended as their means of expression. Their overriding motivation was the stewardship of their talents, for they believed "God had intended them to show the southern people the way of light and to compel them to follow its path" (p. 111).

It was in their thoroughgoing defense of slavery that the group demonstrated both its problems and its dilemmas. To succeed in their broad program of reform they needed public support, and that induced them to take the lead in the currently popular effort to justify human bondage. But because their goal was to transcend the limitations of their own society and deal with eternal verities, their essays in support of slavery wedded them irrevocably to their small world. It is the author's thesis, one worthy of consideration, that in portraying an idealized slave society in words which they knew to be false, they were trying not to describe the South but to inspire it. "The only way to legitimate slavery, their arguments implicitly warned, was to transform the region into the moral utopia of their essays. The proslavery argument was thus in essence a charter for reform" (pp. 121-22).

This is a provocative book, full of insights no brief account can adequately describe. It is written

with charm and is thoroughly researched. It is a valuable contribution not only to the history of the South, the American mind, and slavery, but to American letters as well.

DAVID L. SMILEY
Wake Forest University

STANLEY SIEGEL. *The Poet President of Texas: The Life of Mirabeau B. Lamar, President of the Republic of Texas.* (Presidents and Governors of Texas Series.) Austin, Tex.: Jenkins Publishing. 1977. Pp. 176. \$9.50.

Mirabeau Buonaparte Lamar—what a sonorous and romantic name. How fitting that its wearer should have been a dashing and visionary Texan of the very sort that made the early history of the Lone Star Republic so turbulent, erratic, and colorful.

In this little biography (which, in truth, is hardly more than a lengthy biographical sketch) Stanley Siegel brings to the reader the major episodes of Lamar's career. It tells how he left his native Georgia for Texas in the summer of 1835, returned briefly to Georgia the next year, then arrived back in Texas just in time to become a hero in commanding the Texan cavalry in the battle of San Jacinto. The following year he was elected vice-president of the infant republic along with Sam Houston as president, but soon broke with Houston and two years later himself became chief executive. As president he gained diplomatic recognition for Texas, laid the groundwork for the establishment of a public university and a system of public schools, and expelled the Cherokee Indians from the Texas lands which had been promised them by their adoptive tribesman, President Sam Houston.

The book reveals also how Lamar's ambitions ultimately overtaxed the resources of the new nation and sank it into virtual bankruptcy and how Lamar toward the end of his term spent long periods back in the United States, taking medical treatments and leaving the affairs of Texas in the hands of Acting President David G. Burnet. Then comes a summary view of Lamar's later years, which included brief but gallant service in the Mexican War, a stint in the Texas state legislature, and a few months as American minister to Nicaragua. Finally, occasional mention is made of his endeavors as a poet—glimpses of another side of his life indicated in the title of the book.

This volume is one of a series being edited by Dorman H. Winfrey on the presidents and governors of Texas. As such it fulfills its purpose, because these works are not intended to be definitive studies. This purpose is amplified in the author's

preface by an expression of hope that the book will prompt further investigation into the subject's life and career. It leaves ample room for such investigation. It is a workmanlike narrative, but it is neither interpretive nor critical, and it is too cursory to provide the intimacy in detail or the illumination of personality vital to a genuine biography.

CHARLES P. ROLAND
University of Kentucky

JAY P. DOLAN. *Catholic Revivalism: The American Experience, 1830-1900.* Notre Dame, Ind.: University of Notre Dame Press. 1978. Pp. xx, 248. \$10.00.

Drawing upon a tradition which went back to the sixteenth century, the Catholic Church in nineteenth-century America encouraged priests with special oratorical skills to conduct what Protestants would call "revival meetings" but which Catholics called "missions." Prior to 1850 these missions (usually eight or fifteen days) were held in areas where immigrants of Catholic ancestry were living without the regular care of a priest. Their purpose was "to round up" stray Catholics, revitalize their faith, and inspire them to form a parish, build a church, and sustain the faith. They proved to be a very effective means of missionary activity in a country where persuasion rather than tradition or authority was the basis of ecclesiastical order. Like Protestant revivalism in the West, Jay P. Dolan notes, missions were part of "the organizing process" of the Church in an expanding, mobile society. But after 1850, when the number of priests and churches was able to keep abreast of the Catholic influx, the function of the mission served other ends.

While its primary aim continued to be revitalizing the faith and rounding up lapsed or marginal Catholics, the Catholic revival (sponsored chiefly by the Jesuits, Redemptorists, Paulists, and Passionists) became "a maintenance mechanism" (p. 182). Mission preachers brought harmony to quarreling parishes; they encouraged the formation of devotional sodalities and confraternities; they helped raise funds for parochial schools or new church buildings, and above all they gave "a sense of belonging, an identification," security, and courage to a minority group in an alien and often hostile environment. Because Catholics were at the bottom of the socioeconomic ladder in white America, their revival missions resembled those of the small Protestant sects in their emotionalism, their faith-healing miracles, and their message of submission to one's lot on the promise of a better life in the next world (Dolan calls it "the gospel of acceptance" and says it may explain why "Catholics were consistently economic-occupational

under-achievers" [p. 15]). The mission preachers roundly denounced intemperance, dancing, gambling, "snuff-scouring," and reading frivolous novels or heretical (Protestant) works. They preached hellfire and damnation sermons. They attacked the public schools. But above all they held up the true faith of the one true Church outside of which there was no salvation. Catholic revivals were, in short, rallying points for a besieged people, and they flourished mightily until, after 1900, the Church began to enter the mainstream of American life. Thereafter they faded into the background or were transformed into "retreats."

Dolan provides an excellent study of this "Catholic evangelicalism"—its European origins, its statistical growth, its leading practitioners, its mechanisms, its symbolism, its doctrines, and its sociological significance. Like Protestant revivals, the missions were "not the result of a spontaneous, popular religious awakening. They were manufactured events promoted by the clergy and specially calculated to excite the piety of the faithful" (p. 60). But unlike Protestant revivals, they did not culminate in the conversion experience, for the Church required "ratification" of conversion: the revival sermons might quicken souls and bring repentance for sin, but the end result was "to prepare them for the sacraments of penance and communion." The success of a Catholic revival was measured by the length of the lines before the confessional booths and the numbers of confirmations and communions. Crisis conversions were not listed among the final statistics though temperance pledges were.

Like Protestant revivals, Dolan duly notes, the results of missions in church growth were ephemeral. Parish registers do not show significant increases in baptisms, for most who attended were already baptized, and there is "little evidence of a continued upsurge of piety once the preachers left" (p. 141). Still, Dolan contends, like defenders of Protestant revivalism, even a transient spiritual experience ("saint for a day") was important. Most readers will conclude, however, that the defensive or restorative function of Catholic revivalism was probably more significant than its evangelistic function. Dolan indicates that Protestants were occasionally converted, but he also notes that "the missions were primarily parish-oriented and outsiders were not encouraged to attend" (p. 128). In an interesting digression in the final chapter, Dolan shows that the Pentecostal or Charismatic movement within the Catholic Church today is far closer to the spirit and temper of Protestant revivalism than "the sacramental evangelicalism" of the nineteenth-century missions. This is a valuable addition to the study of American religious history which provides impor-

tant insights into the differences between the revivalism of the Protestant majority and that of "the Catholic sub-culture."

WILLIAM G. MCLOUGHLIN
Brown University

BARBARA J. BERG. *The Remembered Gate: Origins of American Feminism. The Woman and the City, 1800-1860.* (Urban Life in America Series.) New York: Oxford University Press. 1978. Pp. xvi, 334. \$14.95.

Historians have scrutinized women's role in antebellum antislavery reform and in organized women's rights efforts, but they have rarely explored women's involvement in urban benevolence, particularly in the many societies centered on women's urban problems like poverty and prostitution. In her study of women and urban reform before the Civil War, Barbara J. Berg has attempted to right the imbalance. Her survey of women's societies is slim, however, and her evidence does not bear the weight of her central argument that American feminism was an outgrowth of women's urban experience rather than of religion, of small-town life, or of antislavery reform, as previous historians have argued. The subject of women's urban benevolence is vast, given its relationship to the history of reform, religion, women, and the city, but Berg only touches on its intricacies.

In the first place, the numbers of women involved in the societies as well as the nature of their membership and leadership remain uncertain. Perhaps a census of the societies' extent in relation to the population of urban women is impossible to reconstruct, but Berg has not made the attempt. Nor does her survey of such societies extend much beyond New York City. Her conclusion that religion was not key to their formation overlooks the strains of approval in ministers' rhetoric, the many women's religious societies founded between the Revolution and the Civil War, and the fact that a "great awakening" occurred in the years after the Revolution, and not just in the 1820s. Her discussion of the culture's dominant attitude toward women (what Berg terms the "woman-belle" idea) pays insufficient attention to the belief in woman's moral superiority, which was powerful justification for their benevolent activities.

Berg's argument that the anxiety of urban women was greater than that of rural women is also open to question, given the greater availability of shops, theaters, newspapers, and general stimulation in cities. Moreover, her account of the spatial and social dynamics of the city is superficial: to accept her thesis that middle-class women in cities regularly encountered poverty and prostitution we

need to know more precisely where middle-class women lived and where prostitution existed. Evidence suggests that by the 1820s a differentiation between home and work existed in many cities and that certain areas had become forbidden to "respectable" women.

Berg's most interesting finding is that feminist rhetoric pervaded the publications of women's benevolent societies. Yet she fails to explore the interaction of these societies and their members with better-known antislavery and women's rights societies. What was their reaction, for example, to Susan B. Anthony's New York campaign in the 1850s for the extension of married women's property rights and for suffrage? Does Anthony's and Elizabeth Cady Stanton's failure to discuss urban benevolence in their *History of Woman Suffrage* indicate an embarrassment at having been outdistanced by their urban counterparts? In general, Berg's work would have sustained a provocative article; but in the brief book format she has chosen, deficiencies in argument and documentation are only too evident.

LOIS W. BANNER
Princeton University

JAMES LEE MCDONOUGH. *Shiloh—in Hell Before Night*. Knoxville: University of Tennessee Press. 1977. Pp. xii, 260. Paper \$5.00.

Shiloh, fought in April 1862, demonstrated one fundamental thing: after a year of on-the-job training, Civil War commanders still were unable to control their armies.

Even at this remove one winces at the Confederates' movement to Shiloh. With all their false starts, confusions, clanging, and general hullabaloo, it is a wonder they found themselves undetected in their assault formations. "This is perfectly puerile! This is not war!" Albert Sidney Johnston is supposed to have exclaimed as he was trying to get his corps in to their weird battle lines. And the same might have been said by (or said about) the Union commanders, though, of course, the instances were different—notably Sherman, who was surprised, and Lew Wallace, who got himself "lost." The butcher's bill and the subsequent recriminations are symptoms of the star-crossedness of the whole business.

James Lee McDonough claims "something special" and an "elusive mystique" about Shiloh that distinguishes it from all other Civil War battles. Perhaps there is, but his claim remains an assertion rather than a developed argument in his book. He also expressly invites comparison with Wiley Sword's *Shiloh: Bloody April*, which apparently was written simultaneously with his book but was pub-

lished three years earlier. Although both books search the requisite sources and are written in the narrative anecdotal manner perfected by Bruce Catton, McDonough's generous invitation is not without hazards. One possibility for invidious comparison may be related to the matter of scale: McDonough permits himself only about one-third of the space Sword used to tell his version of the story. This determined brevity is admirable but is achieved by sacrificing the opportunity to elaborate and thereby provide a richer understanding of what happened at Shiloh. For example, McDonough discusses the Confederate failures at Forts Henry and Donelson and at Mill Springs in a couple of paragraphs. Sword takes a chapter, and we therefore are considerably better able to understand the strategic and personal imperatives that drove Johnston to seek battle at Shiloh. Sword also provides a far superior account of the part field artillery played in the battle (and only on Sword's tactical maps is the artillery designated), as well as answers for such minor but intriguing matters as the location of the foundries that cast Confederate cannon from plantation bells and the provenance of "seeing the Elephant," the idiom for being in combat.

The merits of McDonough's book are its conciseness and its judicious handling of the partisan questions informing the battle. For example, he provides a carefully qualified exculpation of Beauregard and what I take to be the same for Lew Wallace.

With *The Face of Battle*, John Keegan has probably raised battle writing to a new level of penetration and elegance. Although it would be a mistake to take him as a strict model, writers of Civil War military history might profitably look to him for better ways to evoke battle as it was fought midway between Waterloo and the Somme.

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CLEMENT EATON. *Jefferson Davis*. New York: The Free Press. 1977. Pp. xii, 334. \$12.95.

Clement Eaton is one of the leading scholars of the antebellum and Civil War South. We are all in his debt for such brilliant monographs as *The Freedom-of-Thought Struggle in the Old South* and such knowledgeable texts as *The History of the Southern Confederacy* and *The Growth of Southern Civilization*.

But to excel at scholarly texts and monographs is one thing. To excel at biography is emphatically another. As the late Paul Murray Kendall reminded us, biography is, after all, a form of literature whose mission is to recreate a human life, to

capture the warmth and immediacy of a life being lived. Alas, Eaton's *Jefferson Davis* does not even begin to capture the man's life. There is no human drama here, no graphic scenes, no telling quotations, no development of character, and no depiction of complex interpersonal relationships. In truth, this is not a biography at all, but an artless, professorial lecture in which the author constantly occupies center stage, delivering grand assessments of Davis's life and times, referring to himself as "I" in the process, and repeatedly dragging up historiographical problems and controversies. In the text itself, Eaton quarrels with Kenneth M. Stampp's "neoabolitionist point of view" (p. 42), recounts his own communications with William K. Scarborough and Stanley Engerman about Davis's mulatto slaves, and even laments the fact that Republicans and southern whites of the 1850s had no opportunity to read Charles W. Ramsdell's article, "The Natural Limits of Slavery Expansion," which would have instructed them that they were wrangling over "an abstraction" (p. 111). Worse still, the chapters on Davis as Confederate president often read like a textbook, a general history of the rebel South constructed around Davis's name. We are told nothing about his personal travail inside the Confederate White House—his outbursts of temper, anguish, joys, and tribulations, as the Civil War raged around him. Incredibly enough, there are even lapses in scholarship, as for example Eaton's flat assertion that British workingmen embraced Lincoln's Emancipation Proclamation. This cheerfully ignores Mary Ellison's *Support for Secession: Lancashire and the American Civil War* (1973), which demonstrates that a significant segment of British workers in fact repudiated Northern emancipation.

What saddens me is that Eaton does have important things to say about Jefferson Davis as Confederate president, offering an appraisal that is neither harsh nor worshipful. He tells us that while Davis had his faults (his neglect of the home front, his dispersion of troops across the whole Confederacy, his poor judgment of men), he was also capable of growth. In the smoke and fire of the Civil War, Davis freed himself from the incubus of states' rights and became the leading spokesman of an organic Southern nation. This is a valuable interpretation of that maligned and enigmatic man, one that Eaton could have well pursued in a collection of essays—a form of writing that would have allowed him to lecture, analyze, and engage in historiographical disputes without regard for artistic narration. Instead, Eaton has buried a worthy interpretation in a lusterless, pedantic biography.

STEPHEN B. OATES
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Amherst

MARY FRANCES BERRY. *Military Necessity and Civil Rights Policy: Black Citizenship and the Constitution, 1861-1868*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. x, 132. \$8.95.

Mary Frances Berry adds to the debate over whether those Republicans who supported federal civil rights for Afro-Americans during the 1860s did so primarily because of an altruistic belief in Negro rights or mainly because of the exigencies of the times: that is, winning the Civil War required army recruitment of blacks. Berry presents evidence that pragmatic considerations were controlling—that wartime necessity was the pivotal determinant—a view also recently advanced with considerable force and detailed development and documentation by Louis Gerteis and Herman Belz. Historians of Reconstruction have also recently emphasized the role of political pragmatism; some contend that the need to maintain power triggered enfranchisement, first of southern blacks and then of northern blacks.

Berry notes that Lincoln began black enlistments in 1862 because additional recruits were needed. When conscription was introduced in 1863, black recruitment was undertaken in earnest: the law indirectly allowed it, the desire to share the burden of the fighting encouraged it, and manpower needs required it. Thus almost 200,000 black troops served in the Union army along with 600,000 whites. Berry demonstrates that black soldiers fought well, earned equal pay and treatment, and black military service helped secure emancipation and civil equality.

Unfortunately, over one-third of the text is a survey of the frequent prewar exclusion of blacks from military service; complex wartime developments are treated unevenly, often superficially. There are also presentist overtones: it is claimed that the threatening presence of, and pressure by, black soldiers spurred Congress to enact civil rights legislation in 1866 (pp. 91-105). Although archival and congressional sources are tapped, newspapers and especially manuscripts are relatively neglected; secondary works are, however, well covered. Errors exist: names (p. 37), dates (p. 106), and titles (p. 121). There are omissions: scanty attention to reactions by Negroes to wartime issues, little attention to experiences of black sailors, and nothing in the postwar survey about support for the Fifteenth Amendment because of the wartime service of black soldiers. In contrast, Herman Belz (*New Birth of Freedom*, pp. 17-31) incisively discusses both military service and national citizenship, emphasizing political and military pressures.

WILLIAM GILLETTE
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New Brunswick

ROGER L. RANSOM and RICHARD SUTCH. *One Kind of Freedom: The Economic Consequences of Emancipation*. New York: Cambridge University Press. 1977. Pp. xix, 409. \$19.95.

The absence of economic development in the South following the Civil War has been the weighty burden of American economic history for almost a century. The South's role in the history of this nation's economy has far exceeded its importance by any conventional measures, and the extensive bibliography of this field has been lengthened recently by the addition of four books applying quantitative methods to this problem: Stephen De Canio, *Agriculture in the Postbellum South* (1974), Robert Higgs, *Competition and Coercion* (1977), Roger L. Ransom and Richard Sutch, *One Kind of Freedom*, and Gavin Wright, *The Political Economy of the Cotton South* (1978). Ransom's and Sutch's contribution to this literature, *One Kind of Freedom*, is an attempt to explain both the lack of overall development in the South and the slow progress of freedmen in the decades following emancipation.

Ransom and Sutch focus on the cotton South, mainly five states—South Carolina, Georgia, Alabama, Mississippi, and Louisiana—and their sources are a combination of the new quantitative type and the more traditional. In the former category, they have sampled the federal population censuses for 1880 (linking them with agricultural censuses), they have drawn extensively on the Dun and Bradstreet archives, and they have also used various reports from state departments of agriculture. In contrast to the newness of their data, their explanation for the ills of the South is a resurrection of an old hypothesis: "the lack of progress in the post emancipation era was the consequence of flawed economic institutions erected in the wake of Confederate defeat" (p. 2).

The imperfect institutions they detail are tenant farming and the credit system, with the villain being the racist, monopolistic merchant who charged high prices for credit and locked farmers into perpetual debt peonage. Ransom and Sutch employ interest rates computed from cash and credit corn prices, as well as narratives about the impact of lien laws on cotton "overproduction," as their main evidence on the impact of the merchant. The institution of tenant farming, they assert, was partly responsible for the slow growth of the South because tenants, having no long-run interest in the land, engaged in a few long-run investments in it. They cite as evidence the extensive use of fertilizers in the postbellum South and the apparent absence of contracts between landlords and tenants which specified improvements.

The interest rates on advances in corn appear, at first, to justify Ransom and Sutch's strong con-

clusion about the role of the merchant. These rates, averaging about 60 percent for Georgia, were calculated by doubling the implicit interest rate derived from the cash and credit prices, since the loan was assumed to be held for six months. Such a procedure may not be justified if loans were not paid back until much later or if merchant capital was idle for the remainder of the year. An interest rate of 30 percent, while still appearing quite high, is more in line with short-term interest rates in other farm areas. One is disappointed that Ransom and Sutch did not use any merchants' record books but depended entirely on two states' department of agriculture reports. The historian's experience with the merchants' account books reveals generally lower interest rates than are computed with Ransom and Sutch's sources. Most importantly, the merchant could not have exerted a very mighty force on the southern economy, since the department of agriculture reports indicate that farmers did not purchase a substantial fraction of their supplies "on time." Furthermore, many landlords also furnished supplies to tenants, and the merchandizing industry experienced rapid entry during this period.

But a more important issue than the cause of southern economic stagnation is whether the South as a whole actually experienced a slow growth rate in the postbellum era. Although the five cotton states, which are the focal point of this book, had very low rates of growth from 1870 to 1900, the entire South suffered only a once-and-for-all drop in income per capita after the war and then grew at a rate equaling that achieved by the North. (See Stanley Engerman, "Some Economic Factors in Southern Backwardness," *Essays in Regional Economics* [1971].) Low income levels in the South were probably less the result of the merchant, than they were the product of a reduced labor supply following emancipation, as Ransom and Sutch correctly point out, and a loss in economic efficiency with the breakup of the plantations. The low level of income per capita in the postbellum South, in my view, indicates that the South was "overpopulated" relative to other regions in the United States, and the real factor to explain is why blacks did not migrate north until fifty years later.

Students of both history and economics at all levels will benefit from reading this volume and will find it to be easily understood and, at the same time, analytically provocative. I suggest, however, that it be read together with an antidote, Robert Higgs's *Competition and Coercion*, which, although asking the same questions and using many of the same sources, arrives at very different conclusions. A February 1978 conference on *One Kind of Freedom* at Duke University has already underscored its importance, and, although this book may provide

only one kind of answer to the burden of southern economic history, it is an answer to be reckoned with.

CLAUDIA GOLDIN
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HOWARD R. LAMAR, editor. *The Reader's Encyclopedia of the American West*. New York: Thomas Y. Crowell. 1977. Pp. ix, 1,306. \$24.95.

Editor Howard R. Lamar, with the assistance of two hundred contributing authors, developed this encyclopedia during the last decade. This publication makes a welcome addition to the few reference books any serious American historian must personally own. The broad range of interests and topics represented in this work should make it a useful starting point for professional historians, and the Western history buff will find it an invaluable aid.

Arranged alphabetically, the topics on the frontier stage of development range geographically from coast to coast and on into Hawaii and Alaska; topics on the post-frontier stages are limited to the trans-Mississippi West. Chronologically, this reference work covers the period from European discovery to the recent past. Personalities, ethnic and cultural groups, historians of the West, writers of western-theme fiction, and Indian history are only a few of the many aspects of the American West chosen for coverage.

This reviewer identified two weaknesses in the topics chosen. First, some topics are broadly conceived while their component parts are neither cross-referenced nor treated separately; for example, the Ludlow incident is subsumed in the "labor movement" entry. The result—the reader must sometimes search creatively for the information he seeks. This is not a serious problem. Second, Lamar states in his preface that "thematically the *Encyclopedia* embraces the story of Indian-white relations; the diplomacy of American expansion; the overland trails experience; the era of the fur trade, the miner, the cowboy, and the settler; and those western subcultures we call Texan and Mormon." Nowhere is Mexican and Spanish history in the Southwest clearly included in this statement, a fact which in itself is not a problem, but, save for the most obvious entries, the text of the *Encyclopedia* does betray a lack of attention to these topics.

Altogether, this is a work of clear merit which should prove useful to all American historians and students of American history.

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MARGARET LEECH and HARRY J. BROWN. *The Garfield Orbit*. New York: Harper and Row. 1978. Pp. xi, 369. \$15.00.

In his later years James A. Garfield discovered Jane Austen and, with undisguised delight, devoured all her novels. The reason for this attraction becomes clear from this new biography, for in the hands of the late Margaret Leech our twentieth president is himself transformed into a figure from a Jane Austen novel. The result is a charming narrative of coming of age in nineteenth-century America.

Leech portrays Garfield as an earnest, ambitious young man of sentiment, immensely attractive to women, who finds himself the object of a spirited sexual tug-of-war. Among the contestants were his strong-willed, widowed mother, who fought to keep her favorite child from slipping out of her influence, and at least five eager young women who plotted to win his affection. The cast included the pretty but insipid schoolgirl, Mary; the severely intellectual spinster Almeda, who wooed his mind; Maria, the pandering older married woman who pushed him into the willing arms of Rebecca, a liberated woman born into the wrong century; and, finally, Lucretia, who won his hand but not, until years later, his heart. That an impoverished college boy such as Garfield could arouse all this frantic attention is a tribute to the power of his personality and an indication that his later success was not unmerited.

Yet the fact that Garfield ultimately became president of the United States is almost irrelevant to this story, for Leech apparently did not intend to write a conventional biography but rather a social history and family chronicle. I say apparently, for Margaret Leech died with only about a third of her projected manuscript written. What she did complete was too fragmentary to publish and too good to discard. The publishers have ingeniously fleshed it out to make a book of respectable length. Harry J. Brown, coeditor of Garfield's diaries, was enlisted to carry the narrative through Garfield's congressional and presidential years, and a judicious selection of Garfield's own letters was appended to round it off.

The result is not the hodge-podge one might have expected, but a reasonably unified and thoroughly readable biographical study. Even so, Leech's prose style was unique and, despite Brown's best efforts, the discontinuity shows. Furthermore, by the nature of his assignment, Brown was compelled to contribute a more or less conventional narrative, while Leech was interested in creating a psychologically oriented character study. It is not to denigrate Brown's fine contribution to concede that the greater interest lies in the first third of the book. Most readers will regret that

Leech was not spared to complete it along the lines of her original plan.

Most readers will also discover, to their delight, that Garfield, hitherto relegated to the limbo of stuffy obscurity, emerges as a fascinating figure in his own right. This and other recent works devoted to the twentieth president are more than tributes to a neglected man; they are also harbingers of a new and refreshing attitude toward the post-Civil War decades in American history. These years, which are so often given the condescending label "The Gilded Age," can now be seen as a highly creative era which shaped the institutions of modern America, and it is becoming clear that Garfield played a key role in that transformation.

ALLAN PESKIN
Cleveland State University

GERMAINE M. REED. *David French Boyd: Founder of Louisiana State University*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1977. Pp. xiv, 315. \$20.00.

Germaine M. Reed's study—it is not quite a biography—of David French Boyd has all the merits of the published dissertation it is. The author has conscientiously worked through various manuscript collections, in addition to the Boyd papers, and consulted sundry newspapers, plus a shelf of theses and dissertations directly related to the state and university. She has also used to admirable advantage specific secondary works to present a detailed narrative of Boyd's heroic (and personally ruinous) commitment to the state seminary which he converted, almost single-handedly, into the university in 1870.

Like many an ambitious lad of his day he had gone west before the war in search of his livelihood. He left his home in Virginia in 1857, not quite a graduate of the university, to be an engineer in Texas, but he got only as far as Homer, Louisiana, where he took a job as college teacher. In 1859 he moved to the seminary and after serving a two-year stint in the Confederate army returned to New Orleans as superintendent of the seminary, which was mainly a military academy. He had found his life-long love. He served the university faithfully—Reed gingerly says neurotically—as president from 1870 to 1884. In the early years he often served without pay, since Louisiana's Radicals looked askance at his lily-white institution and with rancor upon Boyd's open attachment to white supremacy and the Democratic Party.

Boyd dedicated himself to his school, often promising new professors to pay part of their salary out of his own meager income. As a result of such high-minded practices, Boyd—who was recklessly indifferent to his own money matters—re-

duced his family to poverty. As an administrator he was incapable of compromise; and while his own integrity was impeccable, his rigid certainty in all matters resulted in a host of critics and powerful enemies who brought about his dismissal in 1884. After that, he held a variety of forlorn positions with preparatory academies. His inability to manage his money and to put Louisiana State out of his mind made him into a pathetic figure. He was often broke, reduced to living in run-down hotels as he eked out an existence. Finally, after several abortive efforts to regain his old position, he returned to the university as a professor in 1897, thanks to the generosity of his long-suffering brother, Thomas Duckett Boyd, who had recently become president of the school. David—as Reed refers to him throughout—died a broken, bitter, unappreciated, self-created martyr.

All of this and far more Reed covers compassionately, if dryly. One gets almost no feeling for the man or appreciation of his brave wife and seven hungry children, whom he seldom saw. (Part of the answer: he was married to the university.) A more important criticism is that the book is unnecessarily narrow in focus. There is no attention at all to events or men and institutions elsewhere in the South. The combination of that vacuum and the book's unrelievedly flat prose seriously limits the book's appeal and significance.

BRUCE CLAYTON
Allegheny College

E. WILSON LYON. *The History of Pomona College, 1887-1969*. Claremont, Calif.: Pomona College. 1977. Pp. xviii, 614. \$13.00.

When the Pomona trustees determined in 1969 that the college needed an updated history (earlier accounts were published in 1914 and 1944), they could not have chosen a more qualified person than E. Wilson Lyon to write it. Lyon knew Pomona well, and the Pomona constituency knew him well. He had just retired from an unusually lengthy tenure as president (1941-69) during which two-thirds of the living alumni had been in attendance. In addition to his knowledge of and enthusiasm for Pomona, Lyon brought to his task the skills of an accomplished historian. Early in his career he had taught modern European history at Colgate and had published two recognized works in Franco-American relations.

The Pomona described by Lyon is—and has been—a New England-type college with a highly intelligent student body, a very capable faculty, and an emphasis upon personalized education, value formation, and public service. One of the latest and most westward of the many Congregational colleges to begin in the nineteenth century,

Pomona obtained its founders and early leaders from such institutions as Yale, Oberlin, Amherst, Dartmouth, Andover, Carleton, Beloit, and Grinnell. The college quickly gained an academic reputation comparable to that of its eastern counterparts, and in 1913 it became the first four-year institution west of the Rocky Mountains to win a Phi Beta Kappa chapter. Pomona supported its quality academic endeavors with great success in fund raising, including the development of a life-income program that many other colleges imitated and referred to as "The Pomona Plan." When in the 1920s enrollment had grown to seven hundred with pressure to increase even more, the college leaders seriously sought to find "some plan by which . . . Pomona College may be enlarged to meet the demands of an increasing clientele without sacrificing the advantages inherent in the small college" (p. 237). Consequently, the Pomona officials used the Oxford "cluster college" model as a basis for developing their own group of "coordinated colleges" that shared certain services and facilities. Pomona thus became the founding institution of the Claremont Colleges, which by 1967 enrolled four thousand students in five undergraduate colleges (Pomona, Scripps, Claremont Men's College, Harvey Mudd, and Pitzer) and the Claremont Graduate School.

In general, this is a solid work that will appeal to a wide audience. Lyon provides sufficient personnel, program, and physical plant detail to satisfy the Pomona constituency, and he places the local story in the context of the history of southern California and the nation to a degree that will please the scholarly outside reader. My criticisms are few: in isolated places one might wish for a more thorough analysis of the Pomona developments that have attracted national attention (e.g., the structure of the coordinated colleges) and for a reduction in the space allocated to highly specialized "in-group" interests (e.g., the details of ceremonial programs). Lyon's clear and objective account now replaces the earlier histories of one of the most respected undergraduate colleges in America.

WILLIAM C. RINGENBERG
Taylor University

KENNETH FOX. *Better City Government: Innovation in American Urban Politics, 1850-1937*. Philadelphia: Temple University Press. 1977. Pp. xxi, 222. \$15.00.

This book is a peculiar mixture of helpful insight and jargon-ridden prose. It is a convenient source for scholars on the course of "urban innovation" and a valuable summary of the legal views of city

power via Thomas M. Cooley, John F. Dillon, and Frank J. Goodnow. But it also reflects a fractured and superficial reading of the Progressive period.

The author firmly believes in value-free analysis and apolitical policy. He criticizes those scholars, such as Samuel Hays and others, who were concerned about the question of who governs and not how to govern. Smitten by Max Weber, the bureaucratic revolution, and the how-to-govern public administrators, Kenneth Fox spins out a theory he calls "functional innovation" that is not only value-free but almost history-free as well. His key argument is that a small group of federal bureaucrats in the census bureau, with very little outside help, concocted the mode of urban governance that would dominate the first third of the twentieth century. He strenuously insists that the role of businessmen in all of this was minimal and insignificant. He grudgingly concedes that business gave municipal reformers cost accounting but nothing more. By ignoring time-and-motion studies, the "efficiency movement," the New York Bureau of Municipal Research, and its numerous urban offspring—the city commissioner and city manager innovations—as well as minimizing the monumental contribution of the National Municipal League, it is possible for Fox to weave a theory of functional innovation rising almost *de novo* from the United States Census Bureau.

The significance of *Better City Government* lies less in Fox's ability to defend his view of the origins of functional innovation than in what might be called macro-urban theory. Fox's tracing of the city as a creature of the state to the home rule-ism of the early twentieth century, to functional innovation, and finally to the abortive attempts to formulate a theory for metropolitan government is certainly a useful insight into a usable past. His effort to describe contemporary theory about cities as the "Keynesian-pluralist mode of urban political innovation" is not exactly a winsome attempt at phrase making and not completely convincing, given the fact that Fox has overlooked the political sociology of the Left and its impact on 1960s urban policy.

Fox ends his book with a bugle blast for political scientists to drop their models and pick up the mighty lances of innovation to do battle for the noble cause of municipal government. One wonders where Fox has been for the past two decades during which the tocsin calls for activist scholars rang like runaway fire alarms deep into the night, shattering the peace of academe and sometimes its integrity. Fox justifies his call for scholars to take up the swords of policy and forsake their pens with the assertion that "city bureaucrats cannot effectively generate innovation. . . ." This does not quite square with the major point of his book,

which is that federal bureaucrats did generate innovation which Fox calls "functional innovation."

MELVIN G. HOLLI
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CARL V. HARRIS. *Political Power in Birmingham, 1871-1921*. (Twentieth-Century America.) Knoxville: University of Tennessee Press. 1977. Pp. xvi, 318. \$14.50.

A quarter of a century ago *Community Power and Structure*, Floyd Hunter's study of Atlanta, Georgia, evoked a series of investigations by sociologists, political scientists, and historians into the nature of the decision-making process in American cities. Now, Carl V. Harris has completed an important study of political power in another southern city which synthesizes the differing analytical methods of the social scientists and places their debate in a historical context. Harris also offers a fresh new approach to urban historians who have relied too long on the time-worn categories of "bosses and reformers" to explain turn-of-the-century politics. By way of contrast, Harris focuses his attention on economic and social interest groups within the population of Birmingham and seeks to determine which were most likely to affect the results of the governmental process.

Harris demonstrates a thorough mastery of his local sources. He divides the people of Birmingham into three economic interest groups: the top-ranking one percent, consisting of railroads, utilities, banks, and coal, iron, and steel corporations; the middle-ranking nineteen percent, consisting of prosperous merchants, realtors, small manufacturers, and professionals; and the bottom-ranking eighty percent, consisting of retail grocers, saloon keepers, artisans, and wage earners. Harris then analyzes how these interest groups influenced the results of three governmental actions: extraction of revenue, allocation of service, and regulation of activity.

This case study does not get mired down in local history: it carefully documents parallels between Birmingham and other cities. Where studies elsewhere have shown that economic elites have monopolized elected positions at various times in the nineteenth and twentieth centuries, Harris shows that in Birmingham between 1871 and 1921 public officeholders (as well as candidates for office) were predominantly men from the middle 19 percent of the population. But, more important, Harris demonstrates that "the positional pattern of a city, whatever its shape, might not be congruent with the actual distribution of political power among groups" (p. 281). In fact, the evidence on Birming-

ham indicates that the middle 19 percent did not fare best in the decision-making process; on the contrary, the industrialists were the most successful in influencing governmental processes. Their record of success was not the result of bribery or of "string pulling" but of the structure of the decision-making process itself. The representatives of the largest corporations were able to employ talented lawyers who helped them to manipulate property laws and to maneuver complex issues through local and state legislative and judicial bodies.

Harris's description of politics in Birmingham is designed for social scientists who are acquainted with the power structure debate. As a result, he uses jargon, such as "positional analysis" and "reputational method," which only specialists will understand. Yet, Harris's work will also be of interest to a wider historical audience because of its portrait of the first fifty years of a leading industrial center in the South. Birmingham's economic base was built by the railroad, iron, and coal companies which capitalized on the other regional resource—cheap black labor—to produce the cheapest pig iron in the world in the late nineteenth century. But the rich natural resources did not bring abundant revenue for the fledgling city. In sum, Harris has also written an excellent case study of how the city-building process can occur under most severe financial constraints which is a model for urban historians who seek to present their research to a wider audience.

TIMOTHY J. CRIMMINS
Georgia State University

ALAN D. ANDERSON. *The Origin and Resolution of an Urban Crisis: Baltimore, 1890-1930*. (Johns Hopkins Studies in Urban Affairs.) Baltimore: Johns Hopkins University Press. 1977. Pp. xi, 143. \$12.00.

This insistently ambitious case study renews the tradition of urban history initiated by Sam Bass Warner's *Streetcar Suburbs* and deserves recognition as an indispensable contribution. Alan D. Anderson examines urban growth as the outcome of a complex weave of private and governmental decisions, which respond to and in turn modify the urban environment.

Anderson offers a formal model of growth and response. In the model, economies of growing cities are perennially subject to increasing land prices, both at center city and urban periphery. Rising land prices tempt urbanites to use land more intensively and to increase the ratio of capital to land. This, however, creates external costs (like pollution and congestion), not captured in land prices, that repel both households and firms. At

some "crisis" point, continued city growth demands either that residents accept a decline in the quality of their lives, or that new technologies be implemented. In Anderson's account, such a crisis was resolved in Baltimore in the early twentieth century by the development of a sanitary sewer system and a new transportation system based on automobiles.

Sewers permitted higher intensity land use; automobiles lowered the price of urban land throughout the city. "Transportation and urban services were very good substitutes for each other" (pp. x-xi), since they were responses to the same crises but with different economic characteristics. The market could allocate automobiles to those who could and would pay. But technologies such as sewers involve large, indivisible expenditures with scant opportunity of quick amortization. The city had to build and build massively, since contemporary ideology demanded of municipal services "that everyone should receive approximately the same benefits" (p. 8).

Anderson's model relates these two kinds of responses to the demographic and spatial growth of Baltimore and to trends in the public and private sectors of the local economy. He further offers a rationale for Progressive reform and demands that we face today's urban crisis with a systemic perspective. Both model and application are enormously stimulating. Their flaws should inspire revision.

Anderson's central notion of crisis is fuzzy in meaning and circular in measurement. Formally, crises occur during the process of urban growth at discontinuities in the economically optimum ratio of capital to land; but Anderson suggests more dramatic connotations. Crisis is the moment when "the very viability of the city as an economic institution was threatened" (p. 66) or when "most residents of the city" thought change was needed (p. 31). To indicate crisis, Anderson proposes that we look for "a sharp change in the rate of growth in municipal expenditures" (p. 83) or for innovation in the provision of urban services. But such measures depend on response to indicate crisis; there can be no pseudocrisis.

The author views Progressive reform whiggishly. When he writes that storm drains were "largely applauded by local residents" (p. 70), his footnote cites "several letters from businessmen" reprinted in an official report (p. 128, n. 16). It is of course businessmen and their professional protégés who define "crisis." Anderson forthrightly insists that business sponsorship of reform does not gainsay its general benefit, but his assessment requires demonstration of the very fact of crisis, either by measuring the deterioration of the quality of life or by showing that contemporaries generally recognized it.

The crisis whose resolution Anderson examines was in part the imaginative product of the reformers who conquered it. Thus, he maintains that during the 1880s and 1890s "it appears that the level of services dropped" (p. 83), citing Baltimore's commissioner of health, who asserted that inadequate sewage disposal was "lowering the vital stamina" of Baltimoreans (p. 66). Yet both general and disease-specific mortality in Baltimore were unmistakably declining. Anderson does not mention that the health commissioner's reports regularly included expressions of gratification with improving health, coupled with his admonitions. "Sanitary improvements," the commissioner lamented, for example, "have not kept pace with natural progress" (Baltimore, Commissioner of Health, *Annual Report for 1896*, p. 7). His reports incorporate value-laden expressions designed to enlarge readers' demands for health services, for he was a professional dedicated to improving health, not just staving off deterioration. A fundamentally political process defined crisis and prescribed a response.

This important book demands intense engagement and excites the imagination. Anderson has made it clearer how we may and must understand urban growth and, despite himself, urban politics.

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BRADLEY ROBERT RICE. *Progressive Cities: The Commission Government Movement in America, 1901-1920*. Austin: University of Texas Press. 1977. Pp. xix, 160. \$10.95.

The commission plan emerged out of the reform currents in Galveston, Texas in 1901; six years later it was adopted in Des Moines, Iowa; by 1915 some 420 cities were using the plan. Then the number of commission government cities began to decrease. Bradley Robert Rice applies a theory of municipal diffusion to explain the origins, spread, and decline of the commission plan movement. The result is an important reassessment of American urban reform in the early twentieth century.

Drawing heavily on newspapers and periodicals, Rice modifies the thesis of Samuel P. Hays and James Weinstein that the commission movement was a campaign of selfish business elites for dominance over municipal policy making. Some business groups brought labor representatives into coalitions that sought to revise government organization along new lines of accountability. Others considered "modernization and efficiency as worthy goals in and of themselves" and sincerely felt "that what was good for Main Street was good for the entire city" (p. 71). Even the Socialists did not always oppose the reform. Rice points out that

direct-democracy provisions in commission charters attracted support for the plan from organized labor and Socialist groups. Public-spirited professionals and academic authorities likewise promoted the plan because it offered a variety of devices designed to eliminate machine corruption and make city government more efficient and responsive.

Equally enlightening is the author's analysis of commission government operations. Most commission cities reported substantial economies and elimination of old debts. There was also an expansion of public improvements such as new municipal buildings, street paving, and better fire and police protection. The litmus test of reform government, however, was its ability to meet the city's human needs. Rice shows that commission administrations, while making some community service improvements, did little in the area of social reform and welfare services. This insensitivity to social problems and the reduction in lower-class representation led trade unions and Socialists to oppose commission government. More damaging were the objections of academicians, municipal experts, and businessmen who had advocated the plan. These Progressives are rightly credited with uncovering serious structural weaknesses and partisan politics in the commission system. This critical publicity resulted in the replacement of the plan with council-manager government. Rice might have examined the reform programs of early manager governments. City managers themselves were greatly responsible for the permanence of the new plan. They exercised leadership in welfare policies that directly affected all urban classes.

Rice's study of the commission movement thus reveals much about the achievements and shortcomings of municipal political Progressivism. Formal organization was important, but it could only tip the scales in the direction of, not substitute for, competent and humane governance.

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MARGARET RIPLEY WOLFE. *Lucius Polk Brown and Progressive Food and Drug Control: Tennessee and New York City, 1908-1920*. Lawrence: Regents Press of Kansas. 1978. Pp. x, 194. \$12.50.

Lucius Polk Brown, a professional chemist, served as Tennessee's first Pure Food and Drug inspector from 1908 to 1915, when he assumed the post of director of the Bureau of Food and Drugs in the New York City department of health. He served there effectively until 1918, when the machinations of Tammany politicians drove him from office. This study is a successful comparison of the public

health career of an important if little-known figure in a rural, relatively homogenous state and in the urban environment of New York.

From an old and prominent family, Brown entered public service when Tennessee passed its first Pure Food and Drug law in 1907. As a private citizen he had done chemical analysis for the state's Bureau of Agriculture and after 1903 represented it in the National Association of State and Dairy Food Departments. In 1911 the Association of State and National Food and Dairy Inspectors chose Brown its president. He was thus closely linked with the professional movement for expert control over food and drugs. He exhibited throughout his career the value of determining the public interest by the application of independent, scientific expertise.

Brown built public confidence in his new office and gradually expanded its staff and functions. When he left Nashville, apparently to increase his salary and his stature in the national movement, however, less competent men were appointed. In New York, functioning in a more complex bureaucratic and political setting, Brown was frustrated. The bureau vigorously pursued violators of sanitary food standards until Mayor John F. Hylan took office in 1918. When Dr. Royal S. Copeland became a health commissioner, he worked skillfully to insure that patronage not merit prevailed. Brown fought Copeland's reorganization of the department; failing, he returned to private life in 1920.

Margaret Ripley Wolfe concludes from Brown's career that the relatively simple Tennessee society was more conducive to the application of bureaucratic and scientific values than the complex milieu of New York. In both places, however, the quality of work depended on the talent of the people in charge. The bureaucracies reformers created were "hollow" and susceptible to partisan influence, eventually frustrating the Progressive public health movement.

Future scholarship can turn these conclusions into hypotheses and analyze them in a comparative institutional framework. Such may reveal that Tennessee was atypical; elsewhere the appointment of experts may have been institutionalized. This book does not examine other states or other reform efforts.

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JOHN DITTMER. *Black Georgia in the Progressive Era, 1900-1920*. (Blacks in the New World.) Urbana: University of Illinois Press. 1977. Pp. xii, 239. \$12.50.

The early decades of the twentieth century were critical in the lives of black Americans. In these

years the caste system became more rigid in the South, black mobility and migration to the North accelerated, and significant protest actions laid a foundation for black civil rights demands later in the century. *Black Georgia in the Progressive Era* considers these and other aspects of race relations and black life in an important Deep South state. A short, rather general, but well-written and well-researched book, this work is a volume in the Blacks in the New World series edited by August Meier.

John Dittmer's method is to treat his subject thematically: "Working," "Community," "Racial Politics," "Black Thought," and so on. These chapters provide an overall picture of black life in Georgia which was at best static, but more often deteriorating in the face of increased white oppression. Cities became more segregated in housing and transportation. Work opportunities became more limited. Abuses of the legal system and white violence took a tragic toll. Political participation was violently and almost totally proscribed. And public education and "progressive" reform offered little hope for improvement. The only dynamic black forces emerging from Dittmer's study are the neighborhood unions, black entrepreneurs, and intellectual elites.

Except for the two chapters dealing with "Community" and "Black Thought" (which are the book's two strongest chapters) and the brief attention given to private education, black Georgians are not the actors in this book. The primary subject is the oppressive white caste system. The author shows clearly and forcefully the ways in which this system abused and controlled the black lower caste in Georgia. In the preface and in his concluding remarks, however, Dittmer suggests that his book is "a closer look at black life" as the victims of oppression attempted "to take charge of their destiny" (p. xi). He also praises "those thousands of black men and women who kept the struggle for human dignity alive in Georgia" (p. 211). While giving some attention to these important goals, his book does not focus sharply enough upon black Georgians. Instead, he gives most attention to the white system and the ways in which it victimized blacks as a group. In short, there are still many questions to be asked and answered before we have a full picture of black life in Georgia in the early twentieth century.

While this book's general findings are not particularly new, they do provide an important factual base, and they deal with a time period for which black history has been relatively neglected.

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ARTHUR S. LINK, editor. *The Papers of Woodrow Wilson*. Volume 23, 1911-1912. Princeton: Princeton University Press. 1977. Pp. xiv, 687. \$25.00.

ARTHUR S. LINK, editor. *The Papers of Woodrow Wilson*. Volume 24, 1912. Princeton: Princeton University Press. 1977. Pp. xxi, 609. \$25.00.

These volumes record Wilson's progress from governor to presidential nominee. Speeches, addresses, news reports, and interviews comprise about two-thirds of the text. Nearly all the 500 or so letters concern politics and add nothing new. Few are personal, chiefly the forty-six to Mrs. Peck, his "Dearest friend." He continued to worry about her welfare, deplored their prolonged separation, and dwelled nostalgically on the idyllic days together in Bermuda. Mrs. Wilson's letters reveal a doting but intelligent woman quick to defend her husband's interests in sticky situations.

Volume 23 opens in May 1911 with a 9,000-mile cross-country trip to probe political sentiment. Parrying questions about his own presidential intentions, Wilson addressed many diverse groups along the way and gained encouragement from their enthusiastic reception. In Oregon William S. U'Ren, a high priest of the Progressive movement, explicated the doctrine of the initiative, referendum, and recall, a sovereign panacea of that day. Back home, the governor campaigned vigorously for its adoption in New Jersey and elsewhere. In New York makeshift headquarters under William F. McCombs assisted by McAdoo and others functioned primarily as an information bureau with inadequate funds. Wealthy friends eventually came to the rescue and its political work expanded. Meanwhile Wilson clubs proliferated throughout the land. In October Colonel House volunteered his services, offering to round up the Texas delegation if stories concerning Wilson's anti-Bryan deviations from party regularity since 1896 were untrue. Another Texan encountered trouble organizing a Wilson club because of Wilson's alleged advocacy of common schools for blacks and whites. Such rumors, like numerous others, were promptly scotched.

Summering at Sea Girt, the encampment of the New Jersey National Guard, the governor found time amid his duties as commander in chief to keep in touch with a host of supporters around the country. Later he launched two intensive personal drives—first in the primaries to purge Democratic members disloyal to his reform program in the previous legislative session; then to elect a Democratic legislature to protect those reforms. The Republicans won, despite the fact that he stumped the state for several weeks by auto, often speaking eight or ten times a day. Undismayed, the gover-

nor now went after his ultimate objective—the 1912 presidential nomination.

Volume 24 starts with the Jackson Day dinner in January 1912 at which all the Democratic presidential hopefuls appeared and spoke. Wilson's speech apologizing to Bryan for his 1907 Joline "cocked hat" letter mollified that statesman. About this time he broke abruptly with his former mentor Colonel Harvey, whose Wall Street connections he felt were damaging his progressive image. Harvey in retaliation embarrassed Wilson by spreading tendentious reports about the rupture. The Hearst press embarrassed him further by publicizing offensive passages from his *History of the American People* (1902) decrying the immigrant riff-raff from eastern and southern Europe, an imputation Wilson had difficulty explaining to indignant citizens of Polish, Hungarian, and Italian extraction.

During the preconvention months, when not battling a hostile legislature bent on passing unacceptable laws, Wilson made long speaking tours in search of delegates. He talked sententiously about the merits of democracy, the evils of boss rule, and the iniquities of the Republican tariff. His remedy for abuses like trusts and monopolies lay in bringing to light the misdeeds of corporate wrongdoers. His eloquence won him few votes, however. Without Bryan's support he made little headway in the West against Clark, while to his chagrin he lost most of Dixie to Underwood because of southern distrust of his "radicalism." Despite these setbacks things went better than expected at Baltimore. He awaited the outcome calmly at Sea Girt with a telephone line to Baltimore headquarters. A reporter present observed sardonically that the telephone seldom rang because Wilson's managers were too busy making deals for delegates on the convention floor while the governor kept assuring all around him of his utter abhorrence of such practices. Afterwards he never discussed the matter. The volume ends with Wilson preparing his acceptance speech aboard the yacht of his friend, Cleveland Dodge.

The editors have devised a system, explained in the introduction to volume 24, to eliminate errors incidental to transcribing extemporaneous speeches from newspapers. This works well, yet errors persist (for example, Carlisle for Carlyle and Lovell for Lowell). The introduction even mentions a "suberb reporter" (p. ix). Unnecessary footnoting continues to swell the already formidable proportions of this undertaking. A letter from a friend with six children, all under twelve years of age, elicits a footnote giving the full name of each child, while the index lists each one separately.

SEWARD W. LIVERMORE
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FREDERICK C. GIFFIN. *Six Who Protested: Radical Opposition to the First World War*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. 158. \$9.95.

Frederick C. Giffin's *Six Who Protested* takes us over familiar ground, in describing the reactions of a half dozen prominent radicals—Emma Goldman, John Reed, Max Eastman, Morris Hillquit, William Haywood, and Eugene V. Debs—to the First World War and the repression that it triggered. While sketching the major facts of their lives before the war, his book is largely limited to their war-time efforts, their views of the conflict, and their legal difficulties—their arrests, trials, and convictions for alleged violation of one or another federal statute. Not only is this study severely limited, it suffers from a curious stasis, an arrest that fails to convey historical *process*, to describe and account for the Panglossian optimism of some radicals, to give us something of their commitment to revolutionary idealism before the Bolsheviks emasculated it, or to communicate this time of bewilderment and exaltation stirred in millions by news out of Petrograd in late autumn 1917.

Giffin's sympathies are clear, his prose commendably straightforward and workmanlike, and his attempt to reconstruct this period before Moscow *locuta est* meticulous and humane. But it is often uninspired and, more important, conceptually indifferent. To be sure, it is good to be reminded of his six radicals with their flair for polemic and taste for the grand generalization. They were expressions of the American conscience at its best and most impressive human beings. But Giffin, in this largely uncritical retelling of events, gives us no glimpse of the clash of irreconcilable values, the insufficiency of simple formulas, the poetry of action and courage, or the complexity of men and women and the societies they spoke for. There is little of their noisily intransigent view of the good society and less of their deficiencies and their distinctions in style and substance. And there were distinctions and polemics.

Thus we learn of Debs's antiwar activism and of his trial, but not of the sources of his radicalism—in Populism, Christianity, Marxism, and militant trade unionism—or the difference between it and, say, Goldman's. Nor is there reference to his theoretical weaknesses, shallow dogmatism, or almost mystical sense of self. We read of Hillquit's tactical "good sense," but not of his importance in formulating Socialist Party policy, more so than Debs, and certainly nothing negative about him, such as what Debs called "the cleverness of a pettifogging lawyer." There is Eastman, who was both a Marxist and a Nietzschean and, it follows, more confused about the war's causation than most of the

dissenters appearing here. Ignored, too, is Reed's romanticism, his quest for self, his anti-intellectualism and suspicion of abstract thought (shared by Eastman), and his turn to activism and direct experience. Haywood's expulsion from the Party receives a one-sentence notice, and his concept of socialism and its implications—even more vague than Hillquit's and Debs's—is neglected; so, too, is his readiness, unlike Goldman's, to use the ballot box.

These densely woven, contradictory themes and possibilities for interpretation and comparison are passed over. We can appreciate the self-restraint that produced this tight, thematically unified little book while lamenting the loss that a wider perspective would bring; and Giffin, it should be acknowledged, displays a grasp of the material that would have made this more valuable approach possible.

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ESTHER VOLLES FELDBLUM. *The American Catholic Press and the Jewish State, 1917-1959*. New York: Ktav Publishing. 1977. Pp. xx, 199. \$12.50.

For most of the last nineteen centuries, relations between Christians and Jews have been mainly hostile, relieved only by rare periods of wary, albeit peaceful, coexistence. Time and again Jewish communities have been ravaged by their gentile neighbors, with massacres carried out in the name of the cross. As numerous Christian scholars have shown, the Holocaust was the logical extension of centuries of anti-Semitism endorsed by Christian churches. The unparalleled cruelty of Nazi Germany, however, shocked a number of Christian theologians into a reappraisal of traditional attitudes toward Jews, a process later endorsed by Vatican II.

At the same time as this rethinking was taking place, the Jewish people added a new factor to the debate, the successful drive to reestablish an autonomous Jewish state in Palestine. The resettlement of Jews in the Holy Land confused many Christians, especially Catholics, who found Zionism and its accomplishments contradictory to established doctrine regarding the punishment of the Jewish people for its alleged deicide.

In her doctoral dissertation, Esther Volles Feldblum carefully examined official Church documents as well as a large sampling of the American Catholic press to chart the often confused reaction of Catholics in the United States to the rebuilding of Zion, from the Balfour Declaration in 1917 through the 1956 Suez campaign. Her tragic

death in an auto accident cut short her planned expansion of this study, but her friends at Columbia University saw to it that her work, admittedly incomplete, was published. It is a fine book, and one can only lament the loss of a promising young scholar and those studies which she might have written.

Feldblum critically yet compassionately explored the internal inconsistencies of American Catholic thought. On the one hand, Israel seemed a rebuke, a denial of centuries of accepted Catholic dogma which held that Jews had been exiled and would be forever punished for the death of Jesus; the idea of Jews controlling a land filled with Christian holy places was inconceivable. On the other hand, simple and decent humanitarianism could hardly deny a persecuted people a place of refuge, a country they could call their own.

These problems are still not worked out. The Jewish-Christian dialogue received a severe setback after the June 1967 war, when Jews perceived how few Christians had any real idea of the meaning of Israel in contemporary Jewish life. Despite a few strong pro-Israel voices in the Catholic camp, the attitude of the Church has been at best ambiguous. The dialogue, fortunately, is continuing, but, in order to understand some of the difficulties involved, we need more books like this one. It is our loss that Feldblum can no longer enlighten us in this area.

MELVIN I. UROFSKY
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GAETANO SALVEMINI. *Italian Fascist Activities in the United States*. Edited and with introduction by PHILIP V. CANNISTRARO. New York: Center for Migration Studies. 1977. Pp. lii, 267. \$15.00.

Gaetano Salvemini, in exile from Fascism, was appointed to the De Bosis Chair in Italian Civilization at Harvard University in 1934, a position which he held until his return to Italy in 1948. During these years in America, he continued his unrelenting struggle against the Fascist regime. Shocked at the favorable sentiment of most Americans toward Mussolini, Salvemini was especially distressed by the inroads Fascist propaganda was making among some four and a half million Italian-Americans. Together with a small contingent of militant anti-Fascists, he sought to combat the pernicious influence of the well-funded and highly organized Fascist propaganda machine. Seeking to overcome American indifference, the anti-Fascists exposed the high-handed methods of persuasion and coercion used by Mussolini's minions. Even the House Un-American Activities Committee was too preoccupied with the Communists and Nazis

to pay much attention to the Fascists. In order to shatter this complacency, Salvemini wrote the present work.

In 1940, Salvemini's pamphlet, *Italian Fascist Activities in the United States*, was published by the American Council on Public Affairs. However, he had in mind a full-length, documentary study which would conclusively demonstrate the extent of Mussolini's American empire and spur the Congress to take action against the Fascist propagandists. Events intervened, and the book remained unfinished and unpublished. Thanks to Philip V. Cannistraro, who discovered the manuscript in Florence, the volume has now seen the light of day. Although the editor has added a preface and conclusion taken from the above pamphlet and has improvised a missing section, the body of the work is as Salvemini left it. Although written with a political purpose in mind, the text is singularly free of polemics or denunciations; rather it proceeds by piling up detail upon detail. Much of the book, in fact, is little more than a directory of organizations and a who's who of the Italian Fascist movement in America. Certainly Salvemini proved that from 1922 on the Fascists were busily recruiting loyal adherents to the Duce and that these activities were carried on under the auspices of the Italian government. By relying upon the reports in Fascist publications and making abundant use of quotations, he convicts them out of their own mouths.

The first part of the volume is devoted to a description of Fascist organizations, especially the Fascist League of North America; the second part treats what Salvemini called "Fascist transmission belts" such as the Order Sons of Italy, the Dante Alighieri Society, and the Italy-American Society; part three focuses upon Fascist mass demonstrations, particularly during the Italo-Ethiopian War. Salvemini suggests that many Italian-Americans were susceptible to the appeal of Fascism because as an ethnic minority they suffered from an inferiority complex; in the "new Italy" of Mussolini they found a source of pride and identity. Curiously this philo-Fascism appeared to be more characteristic of the successful and well-to-do than of the working-class Italian-Americans. Although these are suggestive insights, they are not developed into a systematic analysis of the social composition of the Fascist movement in America. Actually the volume comes closer to being a chronology than a history of the movement because it lacks an overall interpretative framework. Its value then is primarily as a rich collection of raw data, which, however, must be examined critically.

Students of ethnic and political history are indebted to Cannistraro for rescuing this valuable source from oblivion and making it available. He

has also contributed an excellent introduction which places this work in the context of Salvemini's scholarly and political commitments. Through editorial notes, Cannistraro brings to bear the historical research of recent years, including his own, on critical points. Regrettably, the volume is marred by many typographical errors as well as by discrepancies in dates given in the text and footnotes. Since Cannistraro did not have the opportunity to correct the proofs of the book, the responsibility for this deficiency is not his, but that of the publisher.

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VIVIAN GORNICK. *The Romance of American Communism*. New York: Basic Books. 1977. Pp. xiii, 265. \$10.00.

While historians have recorded in detail the actions of American Communist leaders, and charted the deliberations of Party congresses, the lives and convictions of rank and file Communists has been pretty much left to psychologists and sociologists, who generally equate membership in the Communist Party of the United States with deviance, pathology, and even demonology. Presenting interviews with over two score Communists active in the Popular Front period, feminist journalist Vivian Gornick implicitly challenges this viewpoint; not all of her subjects found romance in the Communist movement, but they did discover a context for their lives which "lifted them out of the nameless, faceless obscurity of the soul into which they had been born" and brought world historical significance to their lives of struggle and hardship. Joining the Party in the depression years, most of Gornick's respondents (even those who later left the Communist movement over deep political disagreements) have few regrets about how they spent their political maturity.

The data and conclusions of *The Romance of American Communism* fit into a generally positive, fresh interpretation of the American Communist experience that is emerging from the research of such younger scholars as Mark Naison, Mark Solomon, and Roger Keeran. Apparently unaware of this newer body of scholarship, Gornick doggedly concentrates on the personal side of this experience, and thus her book will have to be supplemented by studies that do not neglect the larger national and international political factors. Yet future historians of the American Communist movement will find it difficult to ignore the perspective on the subject she provides—the view "from the bottom up"—even as they will be obliged to approach her work with considerable caution.

Gornick's cast of characters appears in pseudonymous garb, even when they are, like "Max Bitterman" and "Eric Lanzetti," easily identifiable. She fails to inform her readers where or when they can discover whom she interviewed or whether her sample is in any way representative. More serious is her charge that the price people had to pay for Communist involvement was the ability to be intimate with others. Was this just a characteristic of her sample, or was it the result of a generational gap between the interviewer (exquisitely sensitive, perhaps oversensitive to the emotional valences of interpersonal communication) and her respondents (bred in a more reticent era)? Gornick misses a chance to test her hypothesis of emotional flatness among the Communists by neglecting to probe their relations with the children many of them had; parent-child interactions are usually a litmus test of such matters.

The Romance of American Communism is ultimately as much about the author as it is about the forty-seven Communists whose life stories it presents. Gornick testifies eloquently to the nuances of early life in a Bronx family close to the Communist movement and of drifting away from left-wing political concerns to the literary intensities of "Melville, Mann, Wolfe, and Dostoyevsky" and finally back to a portion of the Communist past as a result of involvement in the women's movements. If her book is a record of the rediscovery by feminists of an organizational heritage in the old Communist movement, then it may suggest ways for the women's movement, under attack as never before in the mid-seventies, to weather the current storms and rededicate itself to sisterhood defined as comradeship.

MARVIN E. GETTLEMAN
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MONTY NOAM PENKOWER. *The Federal Writers' Project: A Study in Government Patronage of the Arts*. Urbana: University of Illinois Press. 1977. Pp. ix, 266. \$11.95.

The Federal Writers' Project (FWP) began in 1935 and lasted some six years before it was dispatched by a Congress that was eager to attack Franklin Roosevelt and the New Deal in politically safe ways and that was on the scent of "Reds" in government. Preoccupation with World War II provided the finishing blow. The FWP and its kindred programs, the Federal Art Project and the Federal Theater, then faded from consciousness. Recently efforts have been made to recall and study these programs in the federal sponsorship of the arts.

Monty Noam Penkower's study is a solid step toward understanding the FWP. Penkower ex-

plains concisely how the FWP was conceived and how it operated, dealt with its many problems, and came to its end. He focuses upon the American Guide Series, the principal product of the FWP. Designed as tour guides, the volumes included essays on a wide variety of topics. They were more than Baedekers, and many offer scholars important information about the American past. Their utility in the 1930s, however, is suspect. They were not cheap, and they were bulky items for a traveler to carry. Moreover, not many Americans could afford to travel in the 1930s.

Penkower notes other problems with the guides. They were uneven in quality and tended to slight touchy topics, such as the treatment of Indians and blacks. Yet, he argues, the guides portrayed blacks more honestly than might have been expected at the time. Generally, the guides avoided the excesses of boosterism and were often surprisingly candid about the realities of America. The guides showed that government could produce writing on sensitive subjects without debilitating censorship.

Penkower also discusses the auxiliary projects of the FWP including the former-slave narratives, the *Life in America* Series, ethnic and folklore studies, and the life-history program of the southeastern regional office. He mistakenly says the last included only laborers. He praises the FWP for providing employment for jobless people, some of whom were very talented writers, but he questions the value of the FWP as a creative vehicle for such authors.

Penkower did extensive research in the Library of Congress, the National Archives, and New York City, as well as interviewing widely. He has used several state studies of the FWP. It is problematical whether he would have gained much from more work in the local and state archives and libraries. More seriously, the study makes little effort to comprehend the cultural milieu of the 1930s in general or the FWP in particular. Finally, Jerre Gelando Mangione's *The Dream and the Deal* and this study overlap in many places.

Penkower took on a large, complex, worthwhile topic and has handled his material in a lucid, well-organized way. The FWP spent more than \$27,000,000, employed as many as 6,000 people at one time in offices all over the United States, and published 1,000 books and pamphlets. Those who want to better understand one of the first attempts by the federal government to support cultural programs are indebted to Penkower.

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ALLAN M. WINKLER. *The Politics of Propaganda: The Office of War Information, 1942-1945*. (Yale Historical

Publications, Miscellany, number 118.) New Haven: Yale University Press. 1978. Pp. x, 230. \$11.95.

This is a brief account of the United States World War II propaganda agency, the Office of War Information. Perhaps because OWI appears to have been rather ineffectual, the author has chosen to slight its methods and achievements in favor of exploring its monumental ambitions and colossal failures. The result is a useful, if limited, study in the weakness of wartime liberalism.

Allan M. Winkler finds the roots and guiding principles of OWI in the idealism of onetime Roosevelt speech writers Archibald MacLeish and Robert Sherwood. Guided by what they mistakenly assumed was the president's vision, MacLeish, Sherwood, and their disciples in OWI, sought to use America's natural monopoly on truth and political purity to raise the "real" (that is, the ideological) issues of the war. OWI was to be herald and missionary of a global New Dealism ushering in the "People's Century."

Unfortunately, neither their objectives nor the strategy of truth had much appeal to most American officials, who saw the war simply as a series of battles to be won as quickly and easily as possible. Nothing as dangerous as the truth, or as suspect as a "People's Century," could be allowed to interfere with the immediate practical concerns of the war. The result was that after a tumultuous and frustrating year marked by a continuous losing battle with reactionary congressmen, cynical journalists, and skeptical generals, OWI lapsed into a spiritless lingering death, its lofty goals reduced to the pedestrian function represented by the safe conduct pass that adorns the book's jacket. Winkler describes this odyssey as a maturation process in which inflated, unrealistic expectations gave way to practical propaganda which "reflected American policy, and indeed, America itself" (p. 157).

The author sees this deflation of liberal idealism as inevitable given Roosevelt's apparent indifference to his putative disciples and his refusal to give OWI the guidance or support it desperately needed. Winkler attributes the president's "detachment" to his "characteristic" singleminded concentration on winning the war and to his readiness to sacrifice subordinates to political expedience (p. 68). But he fails to explore Roosevelt's own views on the needs, objectives, and methods of propaganda and the extent to which he conducted a policy independent of OWI.

Nor does Winkler tell us much about the concerns, as opposed to the hopes, of those responsible for creating and operating OWI. It is difficult to conceive of propaganda policy apart from its maker's sense of public attitudes. In fact, Ameri-

can propagandists from the president down diligently sought out information on what Americans and, indeed, Germans were thinking. Pre-war public resistance to interventionism was at least partly responsible for the administration's initial involvement in propaganda. Even after Pearl Harbor, officials within OWI and outside warned of a persistent reactionary isolationism that could undermine the resolute prosecution of the war and that might be exploited by the president's enemies. These perceptions go to the heart of OWI and the propaganda function in general, but Winkler gives the polls scant reference and attempts little assessment of public attitudes or the role they may have played in shaping OWI's history.

Limitations of this kind do not prevent this from being a creditable introduction to OWI and particularly to its political problems. However, *The Politics of Propaganda* is not the full story of OWI or American wartime propaganda.

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MARTIN V. MELOSI. *The Shadow of Pearl Harbor: Political Controversy over the Surprise Attack, 1941-1946*. College Station: Texas A & M University Press. 1977. Pp. xiv, 183. \$10.00.

Despite concerted attempts to kill them, some political controversies fail to die. Watergate was one such controversy in recent memory. During and after World War II, Pearl Harbor was another. A common thread may link the two, or so at least Martin V. Melosi's volume on the latter would seem to suggest.

To this reviewer's knowledge, Melosi's is the first work to analyze, in scholarly fashion, successive attempts by the Roosevelt and Truman administrations to cope with the domestic political repercussions of the Japanese attack on Pearl Harbor. According to the author, those repercussions included recurrent public and partisan demands to know why and how the surprise attack of December 7, 1941 succeeded, attempts to blame U.S. officials for this success rather than credit the Japanese, and resolves to eliminate the sense of national insecurity that the attack and the subsequent war unveiled. At Pearl Harbor, the author contends, the lingering U.S. quest for "national security" surfaced as a specter amidst the flames (p. 168).

In dealing with this specter, as well as the other problems, the Roosevelt administration decided on a compromise. This involved holding the local commanders in Hawaii responsible for lack of preparedness, deflecting similar charges away from the higher command in Washington, and avoiding as long as possible full public disclosure of the

circumstances surrounding the attack. Sparked by fears that "administration opponents would read too much into the available data and employ it for unscrupulous political attacks" (p. 164), the compromise resulted in a "cover-up" pursued, in the name of national security, through a series of administration-sponsored inquiries, such as the one hastily conducted by the Roberts Commission in January 1942 (pp. 45, 164). Ironically, this compromise produced what it had sought to prevent—a major controversy over the Roosevelt administration's responsibility for the outbreak of war with Japan. Even a long-awaited congressional investigation, conducted with the support of the Truman administration following the war, could no longer dispel entirely the suspicion and mistrust engendered at the outset of the controversy by official explanations that were less than complete.

As suggested above, therefore, the broad links between the Pearl Harbor affair and future political controversies like Watergate would appear to be considerable. But although Melosi's analysis tends to suggest such broader themes, the bulk of his work is rather narrowly and repetitively focused on questions of partisan political advantage, guilt, and blame as they relate to Pearl Harbor in the 1940s. Within this limited purview, his presentation is generally convincing and well researched, though not particularly well written or free from editorial mistakes. The presentation also tends to overlook various connections between Pearl Harbor and certain other pressing issues in U.S. politics between 1941 and 1946: the war in Europe, for example, and questions of U.S. responsibility for an international organization to preserve peace. In terms of this last issue, at least, administration charges of Republican opposition to Woodrow Wilson's League of Nations could often be as devastating, while the fighting raged in Europe, as GOP accusations of Democratic responsibility for the war in the Far East. Melosi's analysis fails to take this consideration into account, particularly in attempting to explain Thomas Dewey's reluctance to pursue the Pearl Harbor issue in his 1944 Republican campaign for the presidency.

RICHARD E. DARILEK
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City University of New York

WILLIAM CHAZANOF. *Welch's Grape Juice: From Corporation to Co-operative*. Syracuse, N.Y.: Syracuse University Press. 1977. Pp. xi, 407. \$20.00.

Although nominally a business history, William Chazanof's study of a well-known food manufacturer is both more and less than is typical of that genre. It is more because the author includes much

personal information about the firm's decision makers and its industrial setting and less because the focus is not on the decision makers and their decisions. The financial records are incomplete for the early period, and, in addition, the company's books remained closed. Although this book encompasses the period 1869–1969, half of the text is allotted to the years since 1945; for this period particularly, the author relies heavily on oral interviews to supplement the written record.

Charles E. Welch, a leading prohibitionist and mainstream Methodist layman, was the innovative entrepreneur during the initial phase of both the firm and its industry. He exemplifies the adage that man does not live by bread alone: part of his motivation in developing grape juice was to provide a nonalcoholic drink. In that sense, Welch did well by doing good because he died a millionaire.

As a result of Welch's flair for product promotion, by 1911 his company was the industry leader with 75 percent of the market; four large producers exercised oligopolistic power over the far more numerous grape growers. While the author notes the antagonism between Welch and the growers, his treatment should have been more sophisticated. To write only that "it was tragic that growers and processors did not study these perplexing questions [price, quantity, and quality of grapes] in concert" (p. 110) is to fail to explicate the inherent clash of interests between buyer and seller with unequal power in the marketplace.

Not long after his death in 1926, Welch's heirs sold the enterprise, and then, in 1945, Jacob Merrill Kaplan, the other central figure in the history of this firm, acquired control. Kaplan resolved the long-standing tension between Welch and the growers by radically altering the interrelationship; he sold the company in 1956 to a grower-owned cooperative. There was thus a shift from private enterprise with decision making vested in businessmen to a cooperative with the farmers at least legally in control even if, owing to the iron law of oligarchy, management remained vested in the managers. This episode is sufficiently unusual to justify the book.

Perhaps because of the unavailability of certain sources, there is an undue amount of material that seems peripheral to the principal theme. The conceptual framework is weak, and business operations are not the author's forte. Nonetheless, he is strong on narrative and ably depicts the transformation of this small scale family firm into a successful modern managerial enterprise.

SAUL ENGELBOURG
Boston University

Foreign Relations of the United States, 1950. Volume 3, *Western Europe*. (Department of State Publication,

number 8888.) Washington, D.C.: U.S. Government Printing Office. 1977. Pp. xiii, 1,840. \$20.00.

Students of the diplomacy of this period will find no surprises in this impeccably edited compendium of cables, minutes, and memoranda. In essence, U.S. foreign policy vis-à-vis Western Europe during 1950 was dedicated to the implementation of containment; whatever judgment one may have about this undertaking, one cannot help but be impressed by the magnitude of the task and the success with which it was pursued. The onset of the Korean War cuts across the period with dramatic effects. After June 25, the tempo of U.S. diplomacy sped up and the pressure for results became inexorable. The material in this volume shows that had European governments not been as convinced as the Americans of the unpredictable nature of Soviet intentions, neither NATO, nor German rearmament, nor European integration would ever have come about.

The common denominator of every U.S. diplomatic advance in Europe in 1950 was clearly the European sense of insecurity vis-à-vis Russia in the absence of American guarantees. Consciously, or unconsciously, the Kremlin's incessant propaganda prophesying the disintegration of the West and backed by threats and menaces contributed in a major way to the success of American foreign policy. Fear of some unpredictable Soviet move was the single most important factor animating the European governments. Had there been no image of Soviet threat there would have been no support either for European integration, or NATO, or German rearmament. If the Truman administration is to be condemned for exaggerating the Soviet danger, it was a view shared by every European government, bar none. At no time did the Kremlin offer Europe the assurances that would, overnight, have dissipated the miasma of fear which alone constituted the principal ingredient for the success of U.S. foreign policy.

Cutting across the diplomacy of 1950 like a transforming force was, of course, the war in Korea. Frustrated until then by the slowness of the European pace, Secretary of State Dean Acheson saw the way opened by the aggression in Korea for the most rapid and dramatic diplomatic advances. Overnight, the European capitals set up a clamor for U.S. military forces to be stationed in Europe that put Acheson in a highly advantageous bargaining situation.

The second most revealing confirmation of this volume is the scope and skill of Acheson's modus operandi as a negotiator. The minutes of the meetings between Acheson and the other NATO foreign ministers are most revealing of his ability to combine the utmost pressure with the utmost suppleness. Rather than have an issue evaded lest it

create unpleasantness, Acheson preferred that the foreign ministers debate it openly and face up to it. Most of the time the logic of the situation favored Acheson's reasoning. The only setback was the French reluctance to rearm Germany.

While willing to give the French time to adjust, Acheson expressed a determination to go ahead with German rearmament, even if the French rejected it. At the same time, when it became evident that Adenauer was trying to exploit Washington's interest in rearmament to extract concessions, Acheson immediately passed the word to John J. McCloy, German High Commissioner, to put the issue on the back burner until Adenauer got over any illusions about what Germany's status really was.

Of course, the fact that the U.S. could deal from strength in so many areas gave Acheson's diplomacy overwhelming advantages. There was a constant search for arenas and contexts within which the momentum achieved by the Marshall Plan and the North Atlantic Treaty could be sustained, and the gains from the evident ability of the West to solve its problems was transformed into the kinds of political and military arrangements that would give credibility to containment and deterrence.

One final note. The tragic emphasis upon the People's Republic of China as nothing but a Soviet puppet appears again and again in the thinking of America's highest officials, including Acheson and General Marshall. Men who had a clear vision about the importance of nationalism in the European context appeared completely unable to understand that China's intervention in Korea was a nationalist reflex.

DAVID S. MCLELLAN
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THOMAS H. ETZOLD. *The Conduct of American Foreign Relations: The Other Side of Diplomacy*. New York: New Viewpoints. 1977. Pp. viii, 159. Cloth \$10.00, paper \$5.95.

Previous accounts of institutionalized aspects of the execution of American foreign relations, mainly through the Department of State and the Foreign Service, were usually written by political scientists such as Graham H. Stuart of Stanford University. A well-trained historian has written this one. Thomas H. Etzold begins with a chapter that summarizes the history of the Department of State from the establishment of the Secret Committee on Foreign Correspondence in 1775 to the passage of the Rogers Act in 1924, characterized as "the most notable legislation ever to affect the organization of American foreign relations" (p. 35). Three following chapters concentrate on developments since 1924. They deal with problems of

organization and bureaucracy, reform efforts, the professionalization of the Foreign Service, the persistence of elitism, the declining influence of the Department of State, the rise in power over foreign policy of military and other agencies, the nature of a diplomatic mission, the life style of its personnel, and the traditional work of diplomats in observing, reporting, and negotiating. The final chapter focuses on contemporary concerns, mainly dissatisfactions, within the diplomatic establishment and what might be done about them.

Specialists in American diplomatic history will find most of the material in this book familiar. Few probably would take exception to the analysis that as diplomats became more professionalized, concern for career overshadowed commitment to service in the larger sense, but this theme too is well known. Yet specialists, as well as others, should find the author's common sense judgments refreshing. He does not, as is common in such books, attack the use of political appointees in diplomatic posts, though he does show that some have been ridiculous. Unlike George F. Kennan, who prefers to have policy making in the hands of professional diplomats, Etzold believes that career officers should recognize their limitations. He views, quite correctly I believe, much that has been written about the historic influence of professional diplomats and secretaries of state on the shaping of foreign policy as "romantic exaggeration." He argues that there never was, as in the view of some professionals, "a Golden Age in which the Department of State, and more especially the Secretary of State, had controlled foreign policy and provided the President's principal advice" (p. 118). Diplomats carry out policies that others make. They should recognize this as their principal task and handle it well.

Aside from these usually sound commentaries, the book's main virtue is graceful synthesis. Although it contains some trivia, such as gossip about diplomats, it tells its story in clear, direct, concise prose, and in a refreshingly skeptical tone. Etzold points out, for example, the absurdity of secrecy in some foreign service seminars when much of the material they use comes from literature available to anyone who will investigate. The volume closes with a useful annotated bibliography of selected printed materials available in good libraries. This account should be valuable to non-specialists who want a brief introduction to the history of the American diplomatic establishment and how it works at present, or to specialists who seek a short readable supplementary book in their courses in diplomatic history.

ALEXANDER DECONDE
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JOHN LEWIS GADDIS. *Russia, the Soviet Union, and the United States: An Interpretive History*. (America and the World.) New York: John Wiley and Sons. 1978. Pp. xiii, 309. Cloth \$12.95, paper \$6.95.

Since the Second World War more attention has been given by American diplomatic historians to relations between the United States and Russia than to any other topic. Condensing this vast amount of material into fewer than three hundred pages is no easy task, yet John Lewis Gaddis has done an outstanding job, and this book should supplant all other surveys of Russian-American relations written in English or in Russian.

Beginning before the American Revolution and ending with the Ford administration, Gaddis argues, along with George Kennan, that "the history of relations between the United States and Russia can be written largely in terms of variations . . . between . . . 'universalist' [ideological] and 'particularist' [national interest] approaches to the problem of achieving security in the world." Cordial relations have been achieved only during "periods in which particularism has prevailed in both countries." Gaddis neglects very little of the secondary material on Russian-American relations, and, where published sources are thin, he has broken original ground.

Acknowledging the contributions of revisionists, he nevertheless downplays the role of American capitalism as a prime impetus for United States actions and policies, while emphasizing the ideological determinants of Russian and Soviet policy makers. If early contacts can be viewed as harmonious, subsequent relations deteriorated by the late nineteenth and twentieth centuries due to the mutually universalist approaches of the leadership of both countries. Particularist policies, on the other hand, can be discovered only in those exceptional instances where mutual survival dictated cooperation. Only now, due in part to the Cuban missile crisis and, more important, to the subsequent American decision (Nixon-Kissinger) to allow the Soviets to reach strategic parity with the United States, have both nations retreated from universalist visions. For, the danger of mutual (if not world-wide) annihilation has left "Washington and Moscow little choice but to embrace particularism."

While Gaddis presents his thesis forcefully, there are points at which the logic of his evidence tends to undercut his conclusions. For instance, arguing that Kennan's 1947 "Mr. X Article" should not be taken as "an accurate description of policies the United States . . . was in the process of implementing," Gaddis suggests that "Kennan had not intended his article as a comprehensive prescription for future action." Yet, others have pointed out

that what Kennan intended may prove less important than State Department interpretations and actions based on his suggestions.

John F. Kennedy is pictured as a successful practitioner of "particularist" policies because he often expressed his "willingness to move beyond ideological rigidities." Yet, Gaddis's own presentation of Kennedy's handling of the Cuban missile crisis and of American Southeast Asia policy attests to the fact that Kennedy's actions were more closely tied to "universalism" than his rhetoric might have suggested. Finally, Gaddis argues throughout that "Soviet analysts have shown a persistent tendency to underestimate the influence of noneconomic considerations of American foreign policy." But in presenting United States reaction to Soviet restriction on the emigration of Russian Jews in 1972, Gaddis admits that "Congress' solicitude regarding the plight of Soviet Jews did not exceed its unwillingness to alienate American farmers by restricting wheat exports." Nevertheless, Gaddis has made an important contribution, which should not be neglected as a starting point for all future studies on Russian-American relations.

HOWARD I. KUSHNER
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DOUGLAS S. BLAUFARB. *The Counterinsurgency Era: U.S. Doctrine and Performance, 1950 to the Present*. New York: The Free Press. 1977. Pp. xxi, 356, \$12.95.

This is the first book from a late 1970s perspective to bring together the "Counterinsurgency Era" as a whole. Douglas S. Blaufarb, a retired CIA employee, has written a well-researched book full of fresh insights into that remarkable period which began early in the Cold War, peaked under John Kennedy, and had its epilogue in Vietnam in the spring of 1975.

The author discusses six insurgencies: the Philippines, Malaya, Vietnam, Laos, Thailand, and Latin America. The chapter on Laos, where the author spent two years, is an interesting, informative, and enlightening account of a situation which is, at best, confusing to a nonspecialist. The real strength of the book, however, is its treatment of Vietnam. Surely after an expenditure of fifty-thousand lives and one hundred and fifty billion dollars the United States can draw some lessons from the "Counterinsurgency Era."

A chapter entitled "The Kennedy Crucible" sets the stage for the later Vietnam experiences by explicating the development of counterinsurgency doctrines by Hilsman, Rostow, and others. The author successfully evokes the naive over-

confidence of those days—easy to see now when the limitations of American power are more obvious. The following chapter brings us through the strategic hamlet failure, and a subsequent chapter covers counterinsurgency efforts after that period. For students of the period nothing new will emerge in those two chapters although they are competently researched and interestingly written.

The chapter "Cords in Charge Vietnam, 1967-1972" is valuable. It is based in part on the firsthand experience of the author, but it is also an excellent analytical treatment of the subject during the high point of the pacification effort in Vietnam (in particular, 1969-70). He correctly identifies the leadership inadequacies of the South Vietnamese government—a politicized military and a shortage of leaders with administrative experience—as the basic problems for which there was probably no solution by that time.

The final chapter, after some digression into Latin America, treats the lessons for the United States of the era and especially of the Vietnam experience. No brief summary would do this section justice, but the author places special emphasis on the problems inherent in getting the American military and civilian bureaucracy to take the kinds of actions necessary in such endeavors as Vietnam. This, Blaufarb stresses, is particularly true when, as in this case, the bureaucrats do not perceive counterinsurgency endeavors as the central mission of their organizations.

The Counterinsurgency Era is an important beginning on a necessary reassessment of a major aspect of the Vietnam War. All students of the period, even those who have read extensively on the subject, will find this work highly useful.

DOUGLAS KINNARD
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R. GORDON HOXIE. *Command Decision and the Presidency: A Study in National Security Policy and Organization*. New York: Reader's Digest Press; distributed by Thomas Y. Crowell Company, New York. 1977. Pp. xix, 505. \$15.00.

Command Decision and the Presidency is better than a popularization, but it breaks no new ground in academic history or political science. It is also more a general review of American defense policy, organization, and legislative-executive relations than a treatise on the role of the commander-in-chief *per se*. A serious student will find it interesting but will get more intellectual meat out of the works of Huntington, Hammond, Schilling, Snyder, Caraley, and May. The book could be useful as a college text, especially in counterpoint to Schlesinger's *Imperial Presidency*; together the books are

an interesting illustration of the reversal of prevalent conservative and liberal views of presidential power.

Much of R. Gordon Hoxie's book is an unevenly organized recapitulation of Cold War history, organizational change in the national security community, *The Federalist*, and recent conservative opinion on strategic choices and congressional excesses. He emphasizes the formative impact of Truman and Eisenhower on the modern defense establishment. Less focus on structure and more on process would have led him to accord more attention to the enduring significance of Robert McNamara's managerial revolution. Indeed, Hoxie curiously devotes more space to discussing the first few months of the Carter presidency than he does to the entire administration of either Kennedy or Johnson. The author is critical of the trend toward congressional constraints on executive discretion, concluding that the president has been emulated in conducting foreign policy.

Hoxie's work is thoughtful but not subtle, opinionated but not unfairly imbalanced, comprehensive but replete with digressions. His warmest feelings are for Eisenhower and Ford (who contributed a foreword). An example of the unevenness of his analysis is his treatment of Douglas MacArthur and the Korean War. Hoxie offers a sympathetic though not exculpatory view of the general, citing the latter's memoirs (*Reminiscences*) freely but making no reference to the superior memoir of policy making on Korea by Army Chief of Staff J. Lawton Collins (*War in Peacetime*) or to the crucial Senate hearings of 1951, *Military Situation in the Far East*. Factual accuracy in the book as a whole is generally satisfactory, though a few trivial errors appear. For example, Maxwell Taylor did not resign from the Eisenhower administration (this misconception is shared by many other observers), nor did he suggest in 1967 that the U.S. should withdraw from Vietnam by claiming we had won the war (the author confuses the general with Senator Aiken), and Richard Nixon did not serve six years in the Navy. Overall, *Command Decision* is a readable survey, not bad but not distinguished.

RICHARD K. BETTS
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MARY SPERLING MCAULIFFE. *Crisis on the Left: Cold War Politics and American Liberals, 1947-1954*. Amherst: University of Massachusetts Press. 1978. Pp. x, 204. \$12.50.

This is a small but ambitious volume on a big topic. Written from what must be described as a

Popular Front perspective, it argues that the Cold War fragmented a formerly united and purposeful left-liberal coalition and in the process led many anti-Communist liberals into a betrayal of the ideals for which they ostensibly stood. The book surveys the debate over Communism within the CIO, the split between the Americans for Democratic Action (ADA) and the Progressive Citizens of America (PCA), Henry Wallace's 1948 Progressive insurgency, various post-1948 exchanges between the "hards" and the "softs" on the Communist issue, the alleged vacillations and capitulations of the American Civil Liberties Union (ACLU), and the activities of the American Committee on Cultural Freedom. It critically examines the Vital Center/pluralist liberalism of Lionel Trilling, Reinhold Niebuhr, Arthur Schlesinger, jr., Max Ascoli, Irwin Ross, and John Kenneth Galbraith; it treats the rise of consensus history in the 1950s as an offshoot of this trend. The new liberals and the consensus historians, Mary Sperling McAuliffe argues, created a false liberal consciousness that abandoned the revolutionary zeal of the Old Left and gave up utopian aspirations for "realism" at home and abroad. Liberal political leaders such as Harry Truman and Hubert Humphrey, for their part, engaged in a needless red-baiting that went far toward creating and sustaining McCarthyism.

Some of the author's specific criticisms strike me as well founded, but the book as a whole is sketchy, selective, overly ideological, and ultimately one-dimensional. Never establishing a national political context on the issue of Communism, it evaluates its characters solely against the author's ideals of principled behavior rather than as individuals attempting to function in real historical situations. Its coverage of anti-Communist liberalism fails to capture the full sweep and motivation of the phenomenon or to do justice to its rationale; whether by design or inadvertence, the less attractive aspects receive emphasis. The *New Leader*, for example, gets more attention than the *New Republic*.

Too many assumptions remain unexplored and too many questions begged to satisfy those who have reservations about the author's argument. She seems to assume that American Communism shared the broader values of the left-liberal coalition and had, in Popular Front days, been responsible for much of its vitality. Yet it is common knowledge that the party was Leninist in its structure, antidemocratic in its attitudes, conspiratorial in its methods of operation, and blatantly subservient to Moscow. It always had coexisted uneasily with the democratic left and had been as frequently a source of disruption as of energy. Typically, its activists concealed their fundamen-

tal allegiance when they sought positions of influence within other organizations.

Liberal anti-Communism, then, had sources other than the Cold War; and, while it is perfectly legitimate to condemn any contributions that some liberals made to the needless persecution of Communists, it is hard to accept the implication that liberals should have accepted them as comrades in a common cause. The author herself reveals the wide gap in values between the Communists and their sympathizers on the one side and the democratic left on the other when she writes that the members of the PCA tended to view Stalinist Russia "as the source of a remarkable and on the whole successful experiment on behalf of human welfare" (p. 7).

Authors are, of course, entitled to their opinions, however much a reviewer may disagree with them. Other readers more in tune with the author will find her work more praiseworthy. Most historians, however, probably will agree that the field has been covered more fully and more satisfactorily by other writers of varying political persuasions.

ALONZO L. HAMBY
Ohio University

MARTIN K. DOUDNA. *Concerned About the Planet: The Reporter Magazine and American Liberalism, 1949-1968*. (Contributions in American Studies, number 32.) Westport, Conn.: Greenwood Press. 1977. Pp. xi, 197. \$14.95.

The history of the liberal political journal, *The Reporter*, provides some significant parallels to the course and logic of the Cold War. When the magazine first began publication in 1949, it strongly supported the Truman administration's anti-Communist foreign policy; before it ended publication in 1968 it strongly defended Lyndon Johnson's policy on Vietnam.

Martin K. Doudna relates the history of *The Reporter*'s origins and development and briefly describes the background and fundamental beliefs of the magazine's founder and editor, Max Ascoli. Under Ascoli's direction *The Reporter* embraced internationalism, opposed both Fascism and Communism (the latter more strongly than the former), and expressed an ongoing concern for individual freedom—a concern predicated on a distrust of unrestricted big government. Its particular mix of guiding principles led *The Reporter* to oppose Senator Joe McCarthy in the 1950s, defend civil liberties and civil rights throughout the postwar period, and, in the 1960s, support the war in Vietnam. Largely because of its position on Vietnam, *The Reporter* lost much of the liberal support it had earlier enjoyed.

But what of the insight *The Reporter*'s history might give the reader about the course of postwar American liberalism? The subtitle promises such a discussion, but the book never fulfills this promise. Doudna, whose own background is in English literature, does not seem at ease writing about twentieth-century American history. He has attempted to provide a broad historic framework for his subject, but he overreaches his realm of expertise. As a consequence, this potentially provocative dimension of the book is disappointing.

Nevertheless, despite the book's limitations, it clearly has its strengths. It presents some basic and sometimes intriguing information about a significant periodical and its influential editor. In particular, the author conducted a series of useful interviews with Ascoli and members of *The Reporter* staff. Many of these interviews are quoted at length and will be of interest to scholars of the postwar period.

MARY S. MCAULIFFE
Iowa State University

RICHARD K. BETTS. *Soldiers, Statesmen, and Cold War Crises*. Cambridge, Mass.: Harvard University Press. 1977. Pp. xi, 292. \$15.00.

The subject of Richard K. Betts's work is the role of military advice in Cold War decisions which involved the possible use of force. In examining what the military recommended, how those recommendations influenced decisions, and the reasons for differing recommendations within the military, the author uses a topical approach to analyze the role of numerous factors, including organizational doctrines and rivalries, direct and indirect advice, personalities, politics, and precedents. His sources include executive documents, congressional hearings, secondary studies, memoirs, journalistic investigations, and numerous interviews.

Betts's conclusions are too numerous, important, and complex to even list, let alone analyze, in a review of this length. He finds that the military has been neither more nor less aggressive than civilians on intervention decisions, but that it has been much more aggressive on ensuing tactical escalation decisions. He also concludes that military influence has been greatest when the Joint Chiefs opposed and least when they favored intervention and that differing recommendations usually find army chiefs most cautious and naval chiefs and field commanders most aggressive.

Although a political science study inspired by the writings of Samuel P. Huntington, this book has great value for military and diplomatic historians, most importantly in its bringing together

into one volume much previously scattered and little-known information on military influence in Cold War decisions. Of more limited value are some of the conclusions. A few belabor the obvious (successful interventions lessen military fears in regard to future interventions), while jargon makes other conclusions virtually incomprehensible ("worst-case military strategy pessimism" versus "bureaucratic revisionist decision-making pessimism"). Equally frustrating, access to the valuable historical information is made difficult by a topical rather than a chronological organization and by discursive footnotes at the end of the text. Moreover, the author has not consulted recently-declassified military records to verify questionable data contained in anonymous interviews, popular magazines, and memoirs.

Betts's work is also marred by a few factual errors and oversimplifications. The Joint Chiefs were created in 1942, not 1939. American isolationism ended before 1941, if indeed it ever existed. Roosevelt vetoed the balanced force concept not only for political and diplomatic reasons, but also because he shared Churchill's belief in the ability of air power to replace large ground forces. "Pacific-firsters" in 1942 included the navy and the majority of the public as well as MacArthur, while the final American strategy was anything but non-political. Revisionist historians neither date the Cold War from 1945 nor blame its excesses on the military; liberal traditionalists do.

Despite its flaws, this is a very informative and useful work. One should note, however, that prior historical knowledge of the Cold War era is necessary to understand the book and to realize its shortcomings.

MARK A. STOLER
University of Vermont

WILLIAM B. QUANDT. *Decade of Decisions: American Policy toward the Arab-Israeli Conflict, 1967-1976*. Berkeley and Los Angeles: University of California Press. 1977. Pp. viii, 313. \$14.95.

This comprehensive study examines American policy and diplomacy from the Six-Day War in 1967 to the election of President Jimmy Carter. William B. Quandt treats the American role in ending the 1967 war; the U.S. peacemaking initiatives in 1967 and 1970; the Jordan crisis of 1970; the three-year period of neglect; the American effort to control the October 1973 war; and the shuttle diplomacy in the aftermath. The readable prose of this book will appeal to the general reader, and the extensive analysis and current bibliography will attract the specialist in the field. It is a balanced study likely to remain a classic in the field for some time.

The decade covered by Quandt is complicated by the growing U.S. dependence upon oil from the Arab world, the ongoing Soviet-American rivalry, and the complicated Arab-Israeli conflict. In analyzing policy making the author posits that the national interest, domestic politics, the bureaucratic machinery, and the presidency all have an interplay in the dynamic of policy formation.

During the June War President Johnson is characterized as moving cautiously because of a growing U.S. involvement in Vietnam and an awareness that Congress would balk at further commitment in the Middle East. In the aftermath of the conflict, Johnson enunciated his five principles for peace, but permitted the Jarring mission full play in its peace-making effort. The Rogers initiatives of 1969 and 1970 are discussed in terms of presidential leadership, domestic politics, and bureaucratic overtures entering the dynamic of policy making.

Quandt describes the 1970 Jordan crisis as more related to Soviet-American competition than to the Arab-Israeli conflict. Nixon and Kissinger are given high marks for management of a crisis in which the American Sixth Fleet and Israeli military force played an integral role in supporting Hussein's regime and containing the Syrian onset. In the aftermath of the crisis, the U.S. viewed Israel as a positive factor in stabilizing the Middle East.

The author characterizes the period from 1971 to 1973 as one of neglect and "lost opportunities to prevent war and move toward a settlement" (p. 129). He suggests that Egypt's initiative in February 1971 could have been fruitful had the Israelis been more tractable.

Quandt asserts that the October 1973 war surprised Nixon and Kissinger, and that the latter delayed sending arms to the Israelis in order to control the conflict and "to induce the Israelis to accept a cease-fire-in-place" (p. 179). U.S. policy makers now turned their attention to the Middle East, for America's allies had been hurt by the crisis, ties with Russia were strained, and the energy crisis was becoming more acute.

In the aftermath of the conflict, the reader is afforded a view of Kissinger's efforts to implement a disengagement and a step-by-step settlement that would restore peace to the Middle East, end the oil embargo, and provide a basis for a continuing diplomatic process. The author concludes with a few general guidelines likely to assist this process.

THOMAS A. BRYSON
West Georgia College

JACK BASS and WALTER DEVRIES. *The Transformation of Southern Politics: Social Change and Political Con-*

sequence Since 1945. New York: New American Library. 1977. Pp. xi, 531. Cloth \$15.95, paper \$5.95.

Updating V. O. Key's *Southern Politics* (1949) has become a cottage industry. How good is the latest product and how does it compare with the original masterwork? Based largely on interviews with prominent southern politicians, knowledgeable newspaper reporters, and a scattering of activists and political technicians, the Jack Bass-Walter DeVries volume is sprightly and readable high journalism. Laced with classic anecdotes, it will intoxicate amateur politicians and aficionados of the regional pastime, flavor undergraduate reading lists, and add zest to lectures on twentieth century politics. Nonetheless, however craftsman-like the sketching of details, analytical scholars will find the canvas as a whole inferior to the original.

In composing his painting, Key employed three types of brushes—interviews, statistics, and more or less self-conscious political theory—and his strokes were both subtle and deft. He transformed political cartography, for instance, by using maps to demonstrate the “friends and neighbors” politics which lent some structure to a system in which the “normal” socioeconomic cleavages were largely suppressed. Although acutely aware of southern diversity and anxious to destroy the stereotype of a homogeneous, unreconstructed South, Key was also a scientist seeking to bring order to his data, to discover correspondences, to generalize. Thus, he found that a politics without parties, and in most states without continuing factions, rewarded certain types of politicians and produced policies that were generally unresponsive to the needs of the lower classes. And Key was not only diligent and perceptive; he was also lucky. Writing at a time when the long-established order was beginning to break down, but before a new one could take shape, he enjoyed a temporal position far enough removed from the old system to afford a clear perspective on it, yet not far enough along in the confused transition to hamper free speculation on the future.

Bass and DeVries, whose first printed words are “V. O. Key” and nearly all of whose chapters include a substantial quotation from Key in the first two pages, are neither so systematic nor so fortunate as their master. Their basic theme is the post-1965 success of white “moderates”—Carter, Bumpers, Askew, and others—usually elected with a firm core of black voting support. The era of “massive resistance” demagogues seems over, the Republican “southern strategy” in tatters, and the rural-dominated inactive legislatures energized by reapportionment. The story unfolds in chapters on each of the ex-Confederate states, four brief chapters on blacks, Republicans, organized labor, and

Congress, and initial and concluding overviews. (An example of their lack of central theme is their arrangement of the state chapters in strict alphabetic order; Key ordered his analogous chapters, which composed less than 40 percent of his text compared to three-fourths of theirs, in a non-alphabetic order to stress particular themes.)

Despite their subtitle (“Social Change and Political Consequence Since 1945”) Bass and DeVries never clearly show just what social changes have taken place in the South, never argue convincingly that it was social, rather than political or economic or ideological, changes that produced the consequences, refuse to generalize about the transformed system, and express only “a cautious optimism” that the metamorphosis they describe is permanent. They never systematically discuss urbanization, industrialization, immigration, or prosperity; they never compare the effects on politics of those developments with the changes produced by Supreme Court and congressional decisions on school integration, black voting rights, and reapportionment. Furthermore, despite the availability of surveys and the huge increase in statistical sophistication in the professional audience since 1949, the present volume is actually less advanced than Key's in its employment of statistics. Their survey questions are too limited, the attitudes probably too transient to be of much interest. A hundred pages of maps, charts, and tables buried in appendixes contain no regressions or correlations, and the authors make extremely arbitrary classifications of counties to be shaded in maps.

Nevertheless, the book is both enjoyable and suggestive, the accounts of particular elections compelling, and the analysis of individual states often extremely prescient. Some of the authors' difficulties arise because current trends in some states are just unclear (what will happen in Alabama, for instance, after Wallace retires?), because they are writing for a popular as much as for a professional audience, and because the inevitable standard against which their work will be judged, Key's classic, is so high. Perhaps we need a series of volumes which are more sophisticated in theory and methodology, but less ambitious in scope, as well as a waiting period until trends become more pronounced before we turn out further facsimiles of Key.

J. MORGAN KOUSSER
California Institute of Technology

GEORGE H. GIBSON. *Public Broadcasting: The Role of the Federal Government, 1912-76*. (Studies in U.S. Economic, Social, and Political Issues.) New York: Praeger Publishers. 1977. Pp. xiii, 236.

George H. Gibson "chronicles the expansion of the role of the federal government in noncommercial educational broadcasting since about 1912" (p. xii). He has no thesis aside from a declaration that federal support is a good thing, no conclusion save for a half-page summary, and no formal bibliography. The author seems to have put a good deal of effort into his book and has made extensive use of public documents, but the result is a disaster. Noncommercial educational broadcasting makes no sense without comparison with commercial ventures in the same area, for federal policy has almost invariably dealt with both commercial and noncommercial access. And to discuss federal regulation of broadcasting without giving examples of programming, describing the relationship of educational mass media to the society in which they exist, or gauging audience response is simply incomprehensible.

Gibson chronicles federal indifference and the zeal of a few interested individuals in a fashion certain to discourage the most diligent reader. Long lists of conference points, committee members, educational station call letters, and tables of acronyms make for slow going at best. The reference to the Office of Education as offering "an anguished cry in its annual report" is exceeded only by the example of that anguish: "'It needs to add personnel of high caliber and to undertake cooperative research programs for the development of television for educational purposes'" (p. 105).

More disturbing is Gibson's failure to consult many basic sources relating to his subject. He makes no use of presidential libraries or the papers of prominent educators and political figures concerned with educational broadcasting. There is no reference to most of the items in the fine bibliographic essay on his topic in Douglass Cater and Michael Nyhan, editors, *The Future of Public Broadcasting* (1976), published in the same series as Gibson's book. There is no mention of similar sources listed in Lawrence Lichty and Malachi Topping, editors, *A Source Book on the History of Radio and Television* (1975) or the bibliographic aids in William L. Rivers *et al.*, editors, *Aspen Handbook on the Media* (1977), first published in 1973. Gibson does not mention Erik Barnouw's three-volume *History of Broadcasting in the United States* (1966-70), and the several references to Sydney Head, *Broadcasting in America* cite the badly-dated first edition (1956), not the superb revisions of 1972 or 1976, both with extensive bibliographies.

It is a mystery to me why a publisher with a series in mass communications would publish a book so deficient in conception and research. Fortunately for historians interested in mass media, Erik Barnouw has just covered much of Gibson's

subject in his marvelous *The Sponsor: Notes on a Modern Potentate* (1978).

DAVID CULBERT
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RICHARD Y. FUNSTON. *Constitutional Counterrevolution? The Warren Court and the Burger Court: Judicial Policy Making in Modern America*. Cambridge, Mass.: Schenkman Publishing Company; distributed by John Wiley, New York. 1977. Pp. xiii, 399. \$17.50.

To attempt, as early as 1976, to write an evaluation of the work of the Warren Court, the wisdom of its judgments, and the net impact of its rulings, *vis-à-vis* the work of the Burger Court to that date, would strike many recent American historians as hubris. For Richard Y. Funston, a political scientist, it is a justified first step in gaining a historical perspective, particularly upon the mistakes and misjudgments of the earlier body. Focusing heavily upon those areas of law which Earl Warren viewed as the most important questions to come before his Court—race relations, reapportionment, the rights of criminal defendants, and to a lesser degree church-state relations and obscenity—Funston attempts to show both continuities and discontinuities in those areas of litigation, contrasting Burger Court behavior with that of its predecessor, and reaching rather specific value judgments on changes in these areas of the law. Such an evaluative process, however, seems skewed from the outset, given the author's overt hostility to liberal judicial activism, his general view that courts should not behave politically, or that if they do, they should carefully mask that fact by convincing the public that their behavior is not political in the same way as that of the other branches. In this regard, his own rather sharply critical assessment of Warren and his Court is largely derived from such scholars as Alexander Bickel, Philip Kurland, and to a somewhat lesser degree, Herbert Wechsler.

One wonders if it is not a bit early to conclude that the Warren Court's struggle to eradicate racial discrimination, both public and private, was largely unsuccessful, at least as a judicial venture; that it pushed the value of equality beyond all reasonable limits, and that the Burger Court, in drawing the line to say "this far and no farther," is performing a commendable public service. Possibly enough time has passed to make similar categorical statements regarding the success of reapportionment, something which Funston does largely on the rate of compliance which has occurred in that area. But to move from this to speculate regarding practical political effects and

view them with considerable skepticism may again be a judgment which might better have been rendered when more of a body of pertinent empirical data is available. Largely ignoring Leonard Levy's devastating assault on the Burger justices as prosecution, result-oriented, conservative activists in the criminal justice area, Funston sees that Court's behavior here as a fortunate form of discontinuity. For him, the Burger Court's perspective on criminal justice, which looks at the totality of circumstances in evaluating the need for extending rights to alleged criminals, is a healthy antidote to the Warren Court's view of the situation from the bottom up, wherein individual interests were the focus. As to the Warren Court's record in church-state relations, it was an area which that body should better have avoided altogether, particularly since the subject was calculated to generate resistance and backlash, factors which damaged the Court's credibility in other areas more central and more vital. Regarding obscenity, recent decisions reveal, "perhaps more than in any other line of precedents, an abrupt change of direction"; a desirable one, however, with the Burger Court's directions resting more clearly on a basis of political prudence.

Thus the Warren Court's record can now be seen as flawed because of its excessive concern with equality as a primary value. Just as earlier courts "had subordinated all other values to that of private property, so too the Warren Court, in its concern for equality became insensitive to the legitimate claims of other competing values." The enormously complex legal, political, and social problems which such an over-obsession produced led Americans to feel that the Court had "gotten out of hand." In this regard, the Burger Court's more realistic view of the limits of its own role seems to the author more practical. Based upon the alternate contemporary assessment of a good many Americans now suffering from the ambiguity and conservative bias of the rulings of that Court, however, the devotion to the achievement of a rather central American value by its predecessor looks, historically, quite good.

PAUL L. MURPHY
University of Minnesota

WALTER LAFEVER. *The Panama Canal: The Crisis in Historical Perspective*. New York: Oxford University Press. 1978. Pp. xii, 248. \$10.95.

PAUL B. RYAN. *The Panama Canal Controversy: U.S. Diplomacy and Defense Interests*. (Hoover International Studies.) Stanford: Hoover Institution Press. 1977. Pp. x, 198. \$5.95.

The ratification of the Panama Canal treaties by the U.S. Senate signified the end of one era in U.S. foreign relations and, one hopes, has opened a new period of effective partnership between the hemisphere's most powerful nation and one of its smallest but pluckiest. Implementing the treaties doubtless will raise questions of interpretation and probably ignite some moments of tension. Although the public and official U.S. interest in Panama characteristic of the past year will wane, it will not fade away entirely. The economic and security links of the United States to the isthmus are too strong for that.

The struggle to ratify the treaties became a bitter war of words. In the emotionally charged debates, ignorance of Panama, of U.S. relations and treaties with Panama, and of the history of the Canal Zone did not inhibit the participants, many of whom seemed determined to do anything except study the treaties and the history surrounding them. Perhaps one can make excuses for slipshod journalism and rhetorical politicians, but it is embarrassing to find bona fide "scholars" who refuse to do their homework and thus contribute to the abundant mythology which has beclouded the public's knowledge and understanding of Panama and the canal.

Under review here are two studies. *The Panama Canal* by Walter LaFeber is the best general study of U.S.-Panamanian relations I have read. It weds solid research with meaningful interpretation. The other, *The Panama Canal Controversy* by Paul B. Ryan, I fear, regurgitates a dreary Cold War drama within a tropical environment; the setting is perhaps different but the plot all too familiar.

Pompous in style, the Ryan book exudes historical errors and dubious interpretations. Castillo Armas did not march on Guatemala City as stated on page 38. The heritage of Spain was not the institutionalization of violence as propounded on page 34. The author is clearly mistaken on the "sovereignty issue" (p. 12), and in later discussions he neglects to mention the clarification of the sovereignty issue in the 1936 treaty and by the 1948 Supreme Court decision. Anti-U.S. sentiments in Panama are blamed at one point on Nazi influence (p. 28) and at another on the Communists (p. 37). Ryan characterizes Panama as unreasonable (p. 36) and stubborn (p. 61) and repeatedly refers to it as one of the "lesser nations," whatever that could mean. Needless to say, no effort was expended to provide the reader with an understanding of the potent force of Panamanian nationalism, and Ryan's contempt for it leads him into some of his attitudes and assertions suggested above. Hoover International Studies has given us an unimaginative book in which all the tired old arguments of the Cold War era are trotted out once again, a

simplistic account far more dangerous in its implications than enlightening. Without new interpretations and insights, this book serves only to reinforce old stereotypes.

On the other hand, LaFeber offers us an exceptionally useful insight into the dynamics of Panama and the difficulties of U.S.-Panamanian relations. With bold interpretations, LaFeber sweeps across Panamanian history from Balboa to Torrijos in six chapters, devoting about half the book to the past quarter of a century. The emphasis falls on U.S.-Panamanian relations but not without providing the readers an excellent background in Panamanian political history. Very importantly, the author understands and deftly explains Panamanian nationalism, that powerful force so little appreciated in the United States but so essential to comprehending Panamanian desires and actions. Extensive archival research and interviews provide a solid foundation for LaFeber's interpretations.

Significantly, the author has read the Panamanian intellectuals and brings some of the Panamanian interpretations into his study, a rarity among writers on the Panama question. The voice of those Panamanian intellectuals has not been heard in the United States even though the treaties sparked one of the major foreign policy debates of the 1970s. Few U.S. scholars, journalists, or politicians have bothered to consult the Panamanian viewpoint; few Panamanian studies have been translated into English. It was a measure of the imperialist mind that we could debate and decide Panama's future without consideration of the Panamanian perspective—I am not asking acceptance of it, merely consideration. The debates accented the ethnocentric exclusiveness characterizing our attitudes not only toward Panama but toward all of Latin America. Thus, *The Panama Canal* makes a refreshing contribution by acquainting its readers with some representative Panamanian viewpoints. The book concludes by tersely and cogently answering five questions frequently raised by opponents of the treaties. The LaFeber book is, in my opinion, the departure point for all future studies of U.S.-Panamanian relations.

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CANADA

C. P. STACEY. *Canada and the Age of Conflict: A History of Canadian External Policies*. Volume 1, 1867-1921. Toronto: Macmillan of Canada. 1977. Pp. 410. \$19.95.

C. P. Stacey, one of Canada's most respected historians, has written the first of a two-volume study tracing the process by which Canada gradually moved from the position of a colony whose external affairs were controlled by the British Foreign Office to the position of a fully self-governing Dominion on the verge of attaining a foreign policy quite independent of Great Britain and the other Dominions.

In his opening chapters Stacey skillfully outlines the problems confronting Canadian governments in their efforts to forge a coherent and acceptable external policy. The primary difficulties were Canada's dependence on the United Kingdom for defense and on both the United Kingdom and the United States for trade; these difficulties were compounded by the British desire to achieve a peaceful, stable relationship with the United States, often at the expense of Canadian interests. These external pressures were paralleled internally by powerful regional and economic diversities and sharp differences in opinion between the English and French communities. In a fluid, commanding narrative Stacey explores how Canada handled these various pressures and worked out a manageable relationship within both the North Atlantic triangle and the emerging Commonwealth. He ranges from the details of the Washington treaty through the tempestuous moments of the Boer War and the naval crisis of 1909-12 to the maneuverings surrounding the abrogation of the Anglo-Japanese alliance.

The major focus of the volume is on the post-1896 period, the years during which Wilfrid Laurier and Robert Borden were prime ministers. Stacey pays particular attention—over half the book—to the impact of the First World War on Canada. As the narrative unfolds, there is an increasingly heavy emphasis on the Commonwealth link, and here Stacey underlines the critically important role of Borden not only in helping to reshape the relationship between Great Britain and the Dominions but also in ensuring that those Dominions, particularly Canada, would have a recognized international status. By contrast, Laurier—forgetful, forever procrastinating, and hampered by his dependency on Quebec—emerges as overly suspicious and dangerously isolationist, unwilling to assume responsibilities and content to wait out events in the security of North America. Laurier did not wish to have a voice in British foreign policy for fear it would involve responsibility while Borden reasoned that, since Canada would be involved anyway, it should have a share in decision making. Canadian losses in the early years of the war and the advent of the Lloyd George government provided Borden with an irrefutable case and a sympathetic ear. With the ex-

pert assistance of the indispensable Loring Christie, he made the most of his opportunities in London and Paris.

This is an important book written with wit and pungency. It is a testament to Stacey's mastery of the existing monographic literature and to his wide-ranging knowledge and judicious use of both government and private manuscripts. It is a fine work of synthesis and original scholarship that places students of Canadian and Commonwealth history in his debt.

JOHN KENDLE
University of Manitoba

JOHN ENGLISH. *The Decline of Politics: The Conservatives and the Party System, 1901-20*. (Canadian Government Series, number 19.) Buffalo, N.Y.: University of Toronto Press. 1977. Pp. x, 237. \$15.00.

The unifying theme of this interesting and valuable study of the Conservative Party of Canada is the attempt by its leader, R. L. Borden, and others to reform and elevate the political process, "to make vision, conflict, and 'creativity' the central features of Canadian politics . . ." (p. 7). Borden shared the dissatisfaction of an articulate minority in the country with the sterile, picayune, parish-pump character of politics, accepting their view that "the significant election issue was not a local bridge or post office, but the future direction of the nation" (p. 5). Thus, John English argues, he rejected the brokerage politics of national unity in favor of a politics of service, purpose, and commitment. At the same time, affronted by the mediocre ability and limited outlook of most of his parliamentary followers, he sought to engage for his party the interest and support of elements outside politics who might assist his efforts at reform. Those efforts failed, English concludes, having, as J. W. Dafoe put it, as little impression on the party system as "a sword-cut in the water" (p. 7).

The theme thus briefly defined in the preceding paragraph is developed with a skillfully managed blend of narrative and analysis. Excellent use is made of a wide array of primary sources and of the rather meagre secondary literature. There is a good treatment of Borden's trying years as leader of the Opposition, but the best part of the book deals with his career as prime minister, especially during the hectic war years. We have here the best scholarly account yet published (the second volume of R. Craig Brown's biography of Borden is yet to appear) of the coming of Union Government in 1917 and of the politics of coalition. There is, as well, a stimulating discussion of the 1917 election, which English calls "the plebiscite on con-

scription," and a very absorbing, suggestive chapter entitled "The Ideology of Service." English is to be commended for adding significantly to our understanding of some rather familiar issues and events.

And yet one has certain reservations, which unfortunately cannot be satisfactorily set out in so brief a review as this. It is open to some doubt that the book's conceptual framework will fully withstand rigorous scrutiny and that the author is always fair and consistent, given the evidence he presents in his interpretation and assessment of Borden's character, motives, and actions and of the government Borden led. For example, it is hard to square the statement that "the coalition which had promised dynamic leadership for a feckless nation had provided none" (p. 218) with this passage: "Its record . . . was not a negative one. It had nationalized and reorganized Canada's bankrupt railways. . . . It had tested the limits of the interventionist state, educating future generations . . . on the limits and possibilities of state activity. A Department of Health, women's suffrage, federal assistance to technical education and to highway construction, and veterans' re-establishment were other valuable legacies" (p. 220).

One could go on at some length detailing particular points on which issue might be joined with English. Nevertheless, he has made a highly useful contribution in a book that amply deserves a careful reading.

ROGER GRAHAM
Queen's University

RICHARD A. PRESTON. *The Defence of the Undefended Border: Planning for War in North America, 1867-1939*. Montreal: McGill-Queen's University Press. 1977. Pp. xiv, 300. \$22.00.

When James Eayrs published his 1964 study of Canadian defense policy between the two world wars, his readers were intrigued to discover that the centerpiece of military planning at Militia Headquarters was a comprehensive scheme for war with the United States. If anyone needed proof of the chronic absurdity of the military mind, surely it could be found in Defence Plan No. 1 and its author, Colonel J. Sutherland Brown.

Among military historians, of course, Eayrs's discovery was only further confirmation of another illusion—the persistent myth of the undefended American-Canadian border. A number of historians, including Charles Stacey, Kenneth Bourne, and the late J. M. Hitsman, have established the importance of the border in British strategy until the Treaty of Washington and the final withdrawal of British garrisons from central Canada in

1871. Richard A. Preston has now brought the story to the outbreak of the Second World War.

Despite the secretiveness of the American military archives and the apparent destruction of the relevant "colour plans," Preston ingeniously establishes that the United States had plans to invade Canada long after "Buster" Brown and his notorious plan had been scrapped. Such documents, he points out, represented the efforts of peacetime armed forces to find meaning for their existence. After Woodrow Wilson's famous 1916 threat to fire any American staff officer he found planning for war with Germany, it may have been more discreet to plan for war with friends rather than with potential enemies.

Thoughts about war across the Canadian-American border were not reserved for underemployed staff officers. Preston offers a rich catalogue of politicians and journalists who joined army officers in thinking about the unthinkable. Before Elihu Root authorized an embryo general staff, ambitious junior officers sharpened their strategic skills by conducting private reconnaissances across the Canadian frontier and turning their findings into invasion plans. On the Canadian side, militia colonels like George T. Denison and Sam Hughes eagerly contemplated their role in any rematch of the War of 1812. Only a few years before he became minister of militia in 1911, Hughes promoted a scheme for a preemptive invasion to seize the Grand Trunk Railway route to Portland, Maine.

One of Preston's most interesting discoveries is that many of the Americans who analyzed the problems of attacking Canada were by no means sanguine about their prospects. Some exaggerated Canada's exiguous military resources; others wrestled with the problems of distance, climate, American public opinion, and the risk of British military intervention. Behind the occasional bluster, leading figures on both sides probably underestimated their respective military potential. "Considerations of this kind," Preston concludes, "probably suggested to thoughtful people . . . that war would be a very uncertain means of achieving political objectives . . ." (p. 8).

This realization came first to the British largely as a by-product of the more systematic strategic planning made possible by the Committee of Imperial Defence. It took longer for both Canada and the United States largely because such planning mechanisms were slower to emerge. It is also true that the Canadian militia, after its contributions to the South African War, 1899-1902, and the First World War, had found a less sterile military function than waiting for a new War of 1812. In due course, the United States Army would do the same.

DESMOND MORTON
University of Toronto

W. A. B. DOUGLAS and BRERETON GREENHOUS. *Out of the Shadows: Canada in the Second World War*. New York: Oxford University Press. 1977. Pp. 288. \$16.95.

When Canada went to war in September 1939, it had a population of about eleven and a half million people. The size of its armed forces was small. The navy was not much larger than it had been in 1914, and one wit described it as a two-ocean navy with a ship in either ocean. In the regular army, counting all ranks, there were enough men to form one small brigade, with no tanks to support it. In the RCAF there were less than two dozen modern fighter aircraft. Despite all the danger signs from Europe, Canada was caught unprepared for war. A decade of economic depression combined with a mixture of apathy and isolationism had stripped the senior Dominion in the British Commonwealth of almost everything but its will to fight.

Six years later Canadians could look back with considerable pride on an immense war effort. Over one million men and women had served in the armed forces. The Royal Canadian Navy, with over 500 ships and over 100,000 officers and men, had played a vital role in the antisubmarine warfare in the Atlantic. Canada's infantry and armored divisions had fought with their British comrades in Italy, France, Belgium, Holland, and Germany. The RCAF, a quarter of a million strong, had squadrons of almost every type fighting from Burma to the skies over Germany. On the home front Canada's industrial and agricultural output surprised even the most optimistic predictions as the country's production of armaments, food, and warlike stores made it a very important contributor to the Allied cause.

By the end of the war the grim memories of the Depression were beginning to fade, and a new confidence was evident in all aspects of life. As the authors put it, Canada had come "out of the shadows" onto the world scene and was prepared to play its new role as a middle power.

W. A. B. Douglas and his colleague, Brereton Greenhous, are professional military historians. Both hold senior positions (Douglas is the Director) in the Directorate of History at National Defence Headquarters in Ottawa. Both have been working primarily on the long-awaited official history of the Royal Canadian Air Force, both have a military background, and this is the first book for both.

To attempt to cover the entire six-year war effort of Canada is no easy task. Yet it has not been done before, and it needed doing. There is no book available which covers in such a sweeping way the Canadian wartime scene at home and abroad. Written primarily for the general public, this well-illustrated and attractive volume has been selected

as a potential favorite by the Book-of-the-Month Club and the Reader's Guild of Canada.

Some Canadian military historians, however, will cock an eyebrow at some of the statements made and conclusions drawn by the authors. To describe Major General E. L. M. Burns as "essentially a staff officer rather than a battlefield commander" is rather severe, as is the authors' claim that, among divisional commanders and above, "only Simonds, Vokes and Hoffmeister could possibly be ranked as first rate." The battles fought by Canadians in Sicily and Italy tend to get disproportionately less coverage than those fought in northwest Europe, and even the bitter fighting to capture the key city of Caen in the Normandy campaign gets about as much space as that devoted to Japanese balloons drifting over British Columbia.

Some of the best parts of the book describe RCAF operations overseas and events on the home front ranging from rationing to wartime propaganda. There are some errors in the book which will probably be corrected with reprinting, but these should not deter the reader who is looking for a good, comprehensive account of Canada in the Second World War.

R. H. ROY
University of Victoria

LATIN AMERICA

MICHAEL CRATON. *Searching for the Invisible Man: Slaves and Plantation Life in Jamaica*. With assistance of GARRY GREENLAND. Cambridge, Mass.: Harvard University Press. 1978. Pp. xxiii, 439. \$32.50.

JEROME S. HANDLER and FREDERICK W. LANGE. *Plantation Slavery in Barbados: An Archaeological and Historical Investigation*. With the assistance of ROBERT V. RIORDAN. Cambridge, Mass.: Harvard University Press. 1978. Pp. xiii, 368. \$20.00.

The re-examinations and reinterpretations of slavery and of slave society that have marked the past decade of work in the United States have had counterparts in the study of slavery elsewhere. As in the United States, with the reopening of many of the questions about the lives of slaves that followed from the scholarly reaction to the interpretation of slavery summarized so forcefully by Stanley Elkins, historians have increasingly used many previously suspect and generally underused sources (such as plantation account and record books and the descriptions provided by plantation owners and their defenders). In addition, because of the frequent absence of written and oral evidence from the slaves themselves, they have applied new techniques and different approaches, including exami-

nations of folklore, archeological reconstructions, and quantitative analysis. These two volumes, each based primarily on the study of one plantation, have ably combined a variety of approaches and techniques in dealing with the depiction of the lives of West Indian slaves and the dynamics of social and cultural adjustment.

Jerome S. Handler and Frederick W. Lange blend a detailed archeological survey with the voluminous manuscript material of the Newton Plantation on Barbados and with traveler and planter descriptions to discuss various dimensions of the slaves' social, cultural, and economic life. The most original aspect of their work for historians of slavery is its archeological investigation, primarily in the plantation cemetery, and the attempts to describe patterns of slave culture based upon inferences drawn from the analysis of ninety-two burials and various artifacts. The reliance on these materials is somewhat uncertain, however, and the book appears more of a suggestive progress report on excavations that were not as successful as hoped for than a conclusive examination of the questions asked. This reflects, in part, the relative novelty of the use of archeological materials to discuss cultural problems and the absence of a broader comparative framework within which to better understand the materials and to test the inferences. Many questions remain, particularly about the dating of the burials and the artifacts, as well as the specifics of the descriptions of various slave practices that are inferred from them. But it seems clear that similar studies, supplemented by more familiar historical tools, can contribute a great deal to the understanding of slave societies. Handler and Lange's basic descriptions of the settlement of Barbados and of the island's slave population are useful, although they could have done more with the available demographic and economic data along lines pursued by Craton. And although the authors place emphasis on what they regard as African patterns surviving in mortuary practices, what may seem just as striking from their presentation are the rather rapid Creolization and adaptations "to the conditions of the plantation system and wider features in the island's sociocultural and physical environments" (p. 215).

Michael Craton's study is based primarily upon the very detailed records of the Worthy Park plantation in Jamaica, which relate mainly to the late eighteenth and early nineteenth centuries. Their use here represents a significant shift in emphasis from Craton's earlier study (with James Walvin), *A Jamaican Plantation: The History of Worthy Park, 1670-1970* (1970), the major focus in this book being the slave population. It is a beautifully produced volume (with numerous illustrations drawn from nineteenth-century sources, in addition to

seventy-one tables, forty-four figures, and fourteen maps) and, of greater importance, a major intellectual achievement. By tracing the lives of slaves and free workers on a Jamaican sugar plantation from the seventeenth century to the present, and by blending several approaches, Craton has made important contributions to the understanding of what it meant to have been a slave and has probed the full meaning of freedom as well.

After a historical overview of the development of Worthy Park, there are three major sections to the book, each concerned with different issues and using varying types of source materials. Although the extent to which this one plantation may be regarded as typical remains, as always, uncertain, Craton is quite sensitive to this point and does provide a reasonable case for his major generalizations. The first, and longest, section uses the extensive slave lists, which provide for each slave (among other information) age, color, place of birth (African or Creole), and occupation; the birth registers, which name mothers; the death registers, which include cause of death; and the slave registration returns from 1817-32. These records contain considerable information on the slave demographic patterns, and the examination of the longer period provides an important complement to the detailed demographic analysis for 1832 presented by Barry Higman. A pattern of an excess of deaths over births persisted on Worthy Park until emancipation, and by separating Africans and Creoles Craton has been able to suggest the relative importance of different forces. Craton points to the role of what he regards as relatively low fertility, arguing that life expectation, particularly for Creoles, was not unusually low. While it is possible that infant mortality (and thus also fertility) was understated, the basic conclusions seem plausible and are consistent with the analysis of Higman and others. This section also includes much useful information on patterns of disease, medical care, occupations, and naming, as well as on the economic performance of Worthy Park.

The second part of the book contains biographies of thirty-four individuals on the plantation, thirty-one slaves and three whites. The author bases these principally upon the information given on the slave lists, supplemented by data from other sources, and uses them to provide descriptions of slave lives and behavior. This approach is, in general, a successful supplement to the detailed quantitative material of the lists, even if at times the biographies are more of a narrative construct from the lists than a presentation of new information. The concluding section traces the initial transition to free wage labor, between 1834 and 1846, on the basis of plantation records. It then leaps forward to an interesting discussion of Worthy Park and its

workers today, based upon a series of oral interviews. This section serves as a powerful reminder of the importance of the postslavery period in shaping modern Jamaica and, by bringing the study up to date, indicates some basic continuities between plantation economies using slave and free labor. While some might find Craton's conclusions a bit overstated, the comparisons of slave and free societies are not always to the advantage of the latter. With its detailed analysis, descriptions, and interpretations, *Searching for the Invisible Man* should become one of the standard works in the study of New World slavery.

Both books under review present new information and insights into slavery as an ongoing system. They are of importance to scholars concerned with slavery in the United States as well as in the West Indies, not only because they offer detailed descriptions of the areas studied, but also because they provide some necessary comparative perspectives for those whose major focus remains the North American mainland.

STANLEY L. ENGERMAN
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ROBERTO CORTÉS CONDE and STANLEY J. STEIN, editors. *Latin America: A Guide to Economic History, 1830-1930*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xviii, 685. \$35.00.

This is an extraordinary book. It is an annotated bibliography of a century of Latin American economic history, but it is a great deal more than that. Its 685 double-columned pages contain 4,552 references in the purely bibliographical sections, and several hundred additional works are referred to in footnotes.

The volume had its origins in the Joint Committee on Latin American Studies of the American Council of Learned Societies and the Social Science Research Council. Work on the book went forward over more than a decade, involving several conferences of the editors and contributors, the individual research efforts of all of these, and the extensive editorial work of Roberto Cortés Conde and Stanley J. Stein. The result is a monumental reference work of major and lasting value.

The *Guide* begins with an introduction by the two editors explaining the origins and nature of the book and a section on "General Bibliography" by Stein (with the aid of Jiřina Rybáček-Mlýnková). These are followed by sections dealing with Argentina (by Tulio Halperin Donghi), Brazil (by Nícia Villela Luz), Chile (by Carmen Cariola and Osvaldo Sunkel), Colombia (by William Paul McGreevey), Mexico (by Enrique Florescano),

and Peru (by Pablo Macera and Shane Hunt). Each of the contributors is a recognized authority on the economic history of the country with which he or she deals.

Each of the sections consists of a preliminary essay and an annotated bibliography. The essays vary somewhat in what they cover but most of them contain both a sketch of the economic history of the country under discussion and a review of the research that has been done on that nation's economic history, as well as an analysis of original documentary sources which are available. Some of the essayists go into considerable detail in describing the libraries, archives, and other places in which material is to be found. Some also outline what the essayists feel to be the most fruitful or necessary fields for further research. These essays themselves would have made a worthwhile book of several hundred pages.

In the bibliographical part of the various sections virtually every book, unpublished manuscript, or primary source is commented upon. Therefore, scholars wishing to use this volume as a guide to their own research can have some notion beforehand of whether a particular source is likely to contain material which would be of interest. Each bibliographical listing is divided into subcategories. This breakdown varies from one country to another, but typical is that dealing with Peru, which includes listings on general and reference works; democracy, manpower, and living conditions; structures and institutions; macro-economic growth and fluctuations; foreign trade and investment; regional economy; agriculture, ranching, and forestry; industry—factory and artisan; extractive industry; and transport, public utilities, and services.

The kinds of material listed are wide ranging. They include government documents of various kinds—ministry and central bank reports, legislative debates and investigations, local and regional government documents, censuses, and many others. Particularly for the earlier decades they include books by travelers who have recorded relevant economic information and observations. There are also numerous documents issued by private organizations of varied kinds, from banks and corporations to labor unions and chambers of commerce, industry and agriculture. The references likewise include published secondary sources, as well as unpublished masters and doctoral theses from the United States, Great Britain, and Latin America.

Both the essays and annotations are written in the native language of their authors. Hence, there are parts of the book in Spanish, English, and Portuguese. The material covered in the bibliographical sections is principally in those three lan-

guages, although there is also a sprinkling of references in French and a handful in German.

The overall impression one receives from reading this volume is one of the tremendous job which still remains to be done before we have a really adequate picture of the economic history of Latin America. A number of the writers themselves comment on this, and the point is underscored by the relatively small number of serious secondary works based on analysis of the kind of material which makes up the bulk of the references. In some of the countries the work of writing their economic history has just begun, in others the subject is virtually a virgin field.

Inevitably, any individual reader will wonder why some material is included in this book, and why other things were left out. This reviewer generally found the parts dealing with organized labor and related problems least satisfactory. But whatever criticisms one may have are merely exceptions which prove the rule: this volume is a magnificent presentation of the most varied sources of information on the economic history of the six leading Latin American nations with which it deals. Although it is probably asking too much of the editors and their collaborators, one is tempted to tell them that one hopes they will go on to produce a companion volume covering the period from 1930 to the 1970s. As it is, scholars in both of the Americas and in Europe as well will long owe a debt of gratitude to Cortés Conde and Stein and their colleagues for what will be an invaluable aid to serious research for many decades to come.

ROBERT J. ALEXANDER
Rutgers University,
New Brunswick

PHILIP WAYNE POWELL. *Mexico's Miguel Caldera: The Taming of America's First Frontier, 1548-1597*. Tucson: University of Arizona Press. 1977. Pp. xi, 322. \$14.95.

Miguel Caldera, mestizo frontiersman of New Spain's Chichimeca borderlands, emerges in Philip Wayne Powell's study as a prototype of Spanish conquest. Son of a Castilian silver miner and a primitive Chichimeca mother, Caldera was able to bridge the gap between two divergent cultures. As a tough, determined, and loyal subject of the crown and an understanding administrator of civil and Indian affairs, in another era he might have gone even farther, but sixteenth-century Mexico was the land of peninsular-born Spaniards.

The silver rush of mid-century conditioned native resistance to northward expansion. Spanish strategy centered upon the use of relatively se-

dentary and Christianized Indians in border defense against hostile, pagan natives. Small presidios, relentless warfare, and a regular application of peace by purchase were employed to keep open the "silver highway" that linked the new mining towns of the Mesa Central. On this frontier Caldera, from obscure beginnings, became an Indian fighter, a peacemaker, a civil administrator, and thereby established himself as "the first of the continent's notable mixed-blood frontiersmen." During the viceregency of Luis de Velasco II, Caldera assumed great importance as chief justice of the pacified Chichimecas, entrusted with the enlightened frontier peace policy. He also built a substantial personal estate consisting of mines, houses, and land.

Superior organization of and persistent pressure by a technologically superior European culture forced the eventual submission of lesser-equipped indigenes. In Mexico much of this process came to fruition during the sixteenth-century mining booms that required peace for successful operations. Dependence, race mixture, and assimilation were the familiar results.

Powell makes comparisons between Spanish Indian policy and later U.S. methods, which, despite the great time difference, invite evaluation. Spain enjoyed greater mobility in military operations, was much more seriously concerned with civilizing and Christianizing, and seldom engaged in the subterfuge of treaties or removal to distant lands. Spain used a basically frontier militia army and held to an ultimate goal of rapid incorporation of Indians into that same army as well as into the colonial system. Caldera was a part of the process, which explains frequent Spanish success as opposed to relative Anglo failure, namely that the Indian became a partner in the development of New Spain, though certainly not an equal one.

Powell's book is well written, the product of lifelong dedication to high standards of professionalism, and based on thorough familiarity with the pertinent archival documentation. Printed clearly with quality maps and illustrations, the book is also esthetically noteworthy. In a day of declining attention to details of book production, this is a pleasant exception, attractive to both specialist and general reader. It is a significant contribution to American history.

DONALD C. CUTTER
University of New Mexico

MARK T. GILDERHUS. *Diplomacy and Revolution: U.S.-Mexican Relations under Wilson and Carranza*. Tucson: University of Arizona Press. 1977. Pp. xiv, 159. Cloth \$10.50, paper \$4.25.

This book is the latest addition to the voluminous literature on United States involvement in the Mexican Revolution. There would seem to be little left to say on the subject, and indeed Mark T. Gilderhus advances no startling new interpretation. He does assemble in one compact volume a competent synthesis of Woodrow Wilson's Mexican policy. Most of the research is from standard U.S. archival sources, and the book is written from the North American perspective, but it is balanced by references to Mexican manuscript materials, notably those in the Archivo de la Secretaría de Relaciones Exteriores. The seven well-written chapters also take good account of important monographs.

To explain Wilson's shifting reaction to the upheaval in Mexico, Gilderhus adopts an "analytical device" (which he credits to N. Gordon Levin) that characterizes Wilson as a "liberal capitalist" who believed that idealism and materialism would be reconciled in a world system based on political freedom and capitalistic enterprise. At the same time, Wilson believed that nations must determine their own destinies, and therein lay the basis of his frustration. At first he looked kindly on the movement led by Venustiano Carranza, who rebelled against Victoriano Huerta the same month Wilson took office. The new president saw in the movement a legitimate quest for constitutional government and self-determination, and he tried to understand and even to help shape it. When his tutelage was rebuffed, when the revolution widened its scope to encompass social and economic changes that endangered U.S. investments, and when its anti-Yankee hostility seemed to threaten U.S. security as the country entered World War I, Wilson's ideals ceased to fit the situation. He abandoned sympathy for the Constitutionalists and retreated to a narrow defense of U.S. interests. In the end he stood pat and left it to his Republican successors to find a nonideological accommodation with the new order in Mexico.

This is straightforward, well-documented history. It is little enhanced by appeal to a "model," which fortunately does not seem to have slanted the author's research although it does underscore his viewpoint. There is an implication here as in other studies that Wilson might have done better if his principles had been different or at least more flexible. But it is also arguable that he might have done worse, and that in fact the "war he kept us out of" was the civil war in Mexico. He withstood pressures from business and religious groups that clamored for wholesale intervention, and his restraint exasperated even his advisers. Carranza, the author might have added, was also a "liberal capitalist," who was impelled by a revolutionary

nationalism that Wilson never understood. Both men were temperamental idealists, but both exercised considerable moderation in the face of repeated provocations. Under the circumstances, both behaved more rationally than has sometimes been recognized.

The book's fine bibliographic essay will interest specialists, and the attractive paperback edition commends itself to instructors in search of a supplemental text for U.S. or Mexican history courses.

DAVID C. BAILEY
Michigan State University

JUDITH ADLER HELLMAN. *Mexico in Crisis*. New York: Holmes and Meier. 1978. Pp. vi, 229. Cloth \$15.75; paper \$7.95.

A decade ago, scholars interested in the problems of economic and political development began to re-evaluate commonly accepted interpretations of the Mexican political system. Until then, the country had generally been considered the site of a burgeoning democratic system, capable of achieving impressive economic growth rates, broad participation in political decision making, and long-term stability in its political processes. Since the student protest movement of 1968 and its forceful repression by the government, however, interpretations of Mexican reality have stressed the authoritarian quality of political decision making, the skewed nature of the distribution of economic rewards, the limited opportunity for opposing the regime, and the importance of the dominant party in controlling dissent.

Judith Adler Hellman has written a basic and readable text which reflects this recent revisionism about the course of Mexican development since the Revolution of 1910. *Mexico in Crisis* surveys the causes and outcomes of the Revolution, the political calculations and opportunism that led to the formation of the ruling party, the dynamics of the Mexican path toward economic development, the mechanisms through which the regime controls opposition, and the potential sources of conflict and change in the future. The book also presents case-studies of the student protest movement in 1968 and peasant organizational activities in recent years. Hellman's argument and evidence indicate that the Revolution of 1910 resulted in a consolidation of political and economic power by bourgeois interests that emerged from the chaos of twenty years of civil strife; that the development policy followed by the regime since the 1940s has benefited middle- and upper-income sectors at the expense of an increasingly marginalized majority

of workers and peasants; and that this path toward rapid economic development has been maintained by a monopolization of power by the government and the official party, the PRI. She concludes that there are few options for changing the course of the Mexican Revolution, short of massive social, political, and economic upheaval, in order to benefit greater portions of the masses of the population. Economic and political elites are firmly entrenched within the system and they have a wide variety of resources available to them for remaining in power.

The scope of this book is comprehensive. It may be faulted, however, for failing to develop sufficiently several important aspects of the dynamics of elite interaction and national decision making in Mexico. While the author explores in a general way the economic development of the country since 1940, incorporation of recent critical thinking about the consequences of "developmentalist" policies—increased rural to urban migration, economic dependence on the United States, and huge balance of payments problems—would have increased the depth of analysis in the book. Similarly, a discussion of changes in political styles and economic policies resulting from changes in administration—the much discussed "pendulum" of Mexican politics—would have been useful. In addition, a clear statement of the individuals and institutions central to national decision making, perhaps drawing on recently published case material and the selection of López Portillo as successor to Echeverría, would have helped enlighten the reader about the inner workings of elite politics in Mexico. In contrast to her discussion of decision making at the national level, Hellman's exploration of the mechanisms of control and co-optation of the masses of the population in order to maintain the current system and its inequitable distribution of the fruits of economic development is cogent and well done.

Students of Mexican history, politics, and economics will be aware that *Mexico in Crisis* does not challenge current interpretations of that country's development or offer data that are not available elsewhere. Instead, the value of this work is in its comprehensive scope, its brevity—the text is 180 pages in length—and its readability. Thus, it is a book that is well suited for classroom use for those who have limited exposure to Mexican development or comparative politics. Hellman has made a useful addition to the basic literature on Mexico and one that can be recommended for use by those teaching in the fields of Latin American political economy, politics, and history.

MERILEE S. GRINDLE
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LAURA RANDALL. *An Economic History of Argentina in the Twentieth Century*. (Columbia Economic History of the Modern World.) New York: Columbia University Press. 1978. Pp. 322. \$21.90.

Laura Randall's interpretation of Argentine economic history in the twentieth century contains several intriguing assumptions about industrialization, economic planning, and dependency. Topically organized, the book, for the most part, focuses on Argentina from the Great Depression through the 1960s.

Vis-à-vis industrialization, Randall's examination of patterns of tariff protection, exchange rates, and price movements convincingly demonstrates that conditions for Argentine growth were more favorable in the 1920s than, as is commonly believed, in the 1930s. World War I, not the Depression, provided the impetus that would transform Argentina "from a supplier of raw materials to a semi-industrialized nation" (p. 5). During the 1920s tariff protection in combination with an expanding Argentine market "led to a tripling of the rate at which new . . . industrial firms were founded and a sextupling of the rate at which foreign industrial firms entered Argentina . . . compared to the rate that obtained during the prior two decades" (p. 125).

Also interesting is Randall's interpretation of economic planning and the policies formulated by various Argentine governments. The pressure generated by competing sectors and groups in their quest for income and wealth "influenced government selection of policies which implicitly determined the profit level of each sector" (p. 9). The argument turns on an analysis of profits rather than prices as the determinant of economic activity. This fresh and ultimately persuasive approach allows Randall to assess from a different perspective the performance and expectations of the multiple sectors of the Argentine economy.

Dependency theorists or Marxists will likely be troubled by Randall's contention that for the last half century "Argentina has not been part of any other nation's economic empire. It has been economically independent in all essentials" (p. 5). Even though Argentina has been described by some as Britain's "Sixth Dominion," Randall argues in common-sense fashion that appearances did not conform to the true dimensions of Argentina's economy or its international economic relations, both of which were infinitely more complex than dependency theorists suggest. Indeed, Argentina's domestic economic sector in the 1920s was three times larger than the export sector and became ten times larger after World War II. Argentine politics and policy formulation responded to

this reality rather than to the alleged machinations of the United States or Great Britain.

However plausible her conclusions, the foundation on which Randall builds is frequently weak. Some sources are biased, others are out of date, and most are secondary. Her chapter on railroads and oil rests heavily on Winthrop Wright's *British-owned Railways in Argentina*, which ignored important archival sources, and Arturo Frondizi's politically inspired *Petróleo y política*. I am not suggesting that Randall's conclusions are wrong. But controversial interpretations demand the best available sources. Data found in the Centro de Documentación of the Argentine Ministerio de Hacienda would have substantially strengthened Randall's arguments, especially with regard to dependency. Randall's book is thus an important addition to the literature on Argentine economic history even though one must use it with care.

PAUL B. GOODWIN, JR.
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KENNETH PAUL ERICKSON. *The Brazilian Corporative State and Working-Class Politics*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xvii, 225. \$14.00.

Until the appearance of this penetrating study, scholars who wished to understand labor's crucial role in the Brazilian political process from the Vargas Revolution of 1930 to the present were forced to fall back on general syntheses of political events, more often than not designed for the textbook-reading college student population in this country. In spite of much excellent publication by Brazilianists in the United States and elsewhere, the political and economic crises that brought on the *golpe* by the military in 1964 have been almost universally shortchanged and misinterpreted. It is disturbing that the only published American historical treatment of labor politics in the 1930-64 period should have remained the standard one for almost thirteen years. Worse still, that study mistakenly characterized the period as an "experiment in democracy," reflecting a fundamental lack of perception for the essentially patrimonial, authoritarian, corporative traditions of Brazilian political life.

Kenneth Paul Erickson's study fills a historiographical void by placing working-class politics in the Brazilian corporative state in its proper perspective, eschewing the tendency to view Brazilian politics through the conventional American liberal prism. He analyzes the ways in which the state managed more or less well to accommodate labor's growing participation in Brazilian politics after the

1930 revolution, and argues that underlying labor's relations with the state was the corporative framework of the Estado Novo erected by Getúlio Vargas. Erickson takes up in succession corporatism and labor, the Ministry of Labor in the corporative state, the political power of the labor leaders, and the state and labor since 1964.

The corporative political system remained viable in Brazil until a new breed of labor leaders began practicing what Erickson characterizes as "dissensus politics" during the Goulart period, wringing concessions from the president by confronting him with actual or threatened electoral realignments. Erickson perceives clearly that Goulart never really understood the labor movement and class consciousness, and saw politics narrowly as trade-offs for opportunistic, short-term advantage; he practiced the old Vargas style. As a result, he lost control of labor. Loss of this control by Goulart frightened military and civilian leaders, since it meant the breakup of the corporative state as then constituted with control over what they considered labor's increasingly extreme demands. Thus, "the process finally culminated in the military overthrow of Goulart's coalition and a restructuring of the political system to exclude left-wing groups from participation in the political process" (p. 99).

This review would be remiss if it did not call attention to Erickson's innovative use of statistical data relating to national expenditures of the Ministry of Labor during the period 1930-64. Drawing upon James Wilkie's pioneering work on Mexican social expenditures, Erickson has for the first time separated official rhetoric from actual labor policy by contrasting projected and real expenditures of the Brazilian Ministry of Labor. Finally, Erickson demonstrates statistically that labor has endured deep sacrifices since 1964, due to losses in real wages as well as forced savings plans instituted by the present regime. It is hard to escape the conclusion that labor has been made the scapegoat for its past radical "sins."

HENRY HUNT KEITH
Golden Gate University

LEONARD E. BARRETT. *The Rastafarians: Sounds of Cultural Dissonance*. Introduction by C. ERIC LINCOLN. Boston: Beacon Press. 1977. Pp. xxiv, 257. Cloth \$10.95, paper \$3.95.

For those unfamiliar with Rastafarians, the term refers to a large but unspecified number of Jamaicans who reject Jamaican national identity. They are loosely tied together by beliefs in repatriation

to Ethiopia as the African promised land for New World black peoples and in Haile Selassie, former emperor of Ethiopia, as the black reincarnated Messiah. The Rasta brethren and their ideology, behavior, and often unique appearance have been the subject of articles since the early 1950s.

The major aims of this work include an account of the development, maintenance, and expansion of the movement; the study of the nature, dynamics, and function of what the author terms a "millenarian-messianic movement"; and the introduction of "Ethiopianism" as the conceptual link between past and present. To accomplish these goals, Leonard E. Barrett devotes three chapters to acquainting the nonspecialist with Jamaica and its people. Unfortunately, his description of the current nature of the society is journalistic. His synthesis of the history of slavery and colonial rule contains several inaccuracies such as the dominant presence of the British in Jamaica before 1600 and the reference to slave participation in the Great (Religious) Revival of 1860. Slaves were emancipated in 1834.

In the remaining four chapters, the author focuses on the Rastafarian movement. When describing and analyzing the beliefs, rituals, and ideology, such as the six basic tenets or the influence of "Ethiopianism," Barrett demonstrates his insightful understanding of religious phenomena. Instead of confining his efforts to such subject matter, he tries to be a social scientist and a historian. He introduces "Ethiopianism," a concept he defines as enchantment and identification by displaced black persons with the fabled Ethiopia of the Old Testament. He terms "Ethiopianism" a historical tradition of which Rastafarian ideology is a logical extension. Yet he pinpoints its origin in 1930 with the preachings of Marcus Garvey. One wonders if forty years constitute a historical tradition.

Barrett's analysis of the Rasta movement using both A. F. C. Wallace's typology and the Gerlach/Hine model of movement dynamics is confusing. He characterizes the movement as "millenarian-messianic" but leaves the reader in a quandary as to whether Haile Selassie or Sam Brown, a local "outstanding leader," is the messiah. Barrett's reliance on Sam Brown as a primary, often exclusive, source of information dominates the book. During this reviewer's research with the Rasta brethren, from 1969 to 1972, Sam Brown was only one of many publicly visible Rastafarians. His writings and poetry were but a small, if even acknowledged, part of similar literary output by movement members throughout Jamaica.

While the author falls into the understandable trap of categorizing movement process and dy-

namics as a static continuum emphasizing ideology or centralized leadership, he is unaware of important factors ignored or unresolved. This reviewer found the Gerlach/Hine model most useful in explaining the Rastafarians. The organization is acephalous and reticulate, recruitment is on a personal basis, individual commitment can be to any part of an ideology that contains few general statements and many local interpretations, and "Establishment" opposition to movement beliefs and goals is a necessary catalyst for membership expansion and widening social impact. Barrett con-

tradicts himself about the organization, does not mention recruitment or the process of commitment, states the belief system as if it were a universal, and ignores the positive function of opposition to the Rasta brethren.

The book is a disappointment in all but one instance. Barrett does furnish solid and extensive descriptive data. On that basis alone he makes a contribution to the literature of social movements and particularly of the Rastafarians.

CLAUDIA M. ROGERS
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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

PHILIP ABRAMS and E. A. WRIGLEY, editors. *Towns in Societies: Essays in Economic History and Historical Sociology*. (Past and Present Publications.) New York: Cambridge University Press. 1978. Pp. viii, 344. \$17.95.

PHILIP ABRAMS, Towns and Economic Growth: Some Theories and Problems. KEITH HOPKINS, Economic Growth and Towns in Classical Antiquity. MARK ELVIN, Chinese Cities since the Sung Dynasty. A. B. HIBBERT, The Origins of the Medieval Town Patriciate. DIANE OWEN HUGHES, Urban Growth and Family Structure in Medieval Genoa. DAVID HERLIHY, The Distribution of Wealth in a Renaissance Community: Florence 1427. CHARLES PHYTHIAN-ADAMS, Urban Decay in Late Medieval England. CHRISTOPHER R. FRIEDRICHS, Capitalism, Mobility and Class Formation in the Early Modern German City. E. A. WRIGLEY, A Simple Model of London's Importance in Changing English Society and Economy 1650-1750. M. J. DAUNTON, Towns and Economic Growth in Eighteenth-Century England. J. J. LEE, Aspects of Urbanization and Economic Development in Germany 1815-1914. E. A. WRIGLEY, Parasite or Stimulus: The Town in a Pre-industrial Economy.

JOSEPH GUTMANN, editor. *The Image and the Word: Confrontations in Judaism, Christianity and Islam*. (Religion and the Arts, number 4.) Missoula, Mont.: Scholars Press, for the American Academy of Religion and the Society of Biblical Literature. 1977. Pp. 175. \$6.00.

JOSEPH GUTMANN, Deuteronomy: Religious Reformation or Iconoclastic Revolution? PAUL C. FINNEY, Antecedents of Byzantine Iconoclasm: Christian Evidence before Constantine. STEPHEN GERO, Byzantine Iconoclasm and the Failure of a Medieval Reformation. ANTHONY WELCH, Epigraphs as Icons: The Role of the Written Word in Islamic Art. WILLIAM R. JONES, Art and Christian Piety: Iconoclasm in Medieval Europe. CARL C. CHRISTENSEN, Patterns of Iconoclasm in the Early Reformation:

Strasbourg and Basel. WILLIAM S. MALTBY, Iconoclasm and Politics in the Netherlands, 1566.

REINHART KOSELLECK, editor. *Studien zum Beginn der modernen Welt*. (Industrielle Welt, number 20.) Stuttgart: Klett-Cotta. 1977. Pp. 393. DM 59.

M. RAINER LEPSIUS, Soziologische Theoreme über die Sozialstruktur der "Moderne" und die "Modernisierung." HANS-CHRISTOPH SCHRÖDER, Die neuere englische Geschichte im Lichte einiger Modernisierungstheoreme. WOLFGANG KÖLLMANN, Zur Bevölkerungsentwicklung der Neuzeit. HANS-JÜRGEN TEUTEBERG, Zur Frage des Wandels der deutschen Volksernährung durch die Industrialisierung. FRIEDRICH-WILHELM HENNING, Der Beginn der modernen Welt im agrarischen Bereich. WOLFGANG ZORN, Verdichtung und Beschleunigung des Verkehrs als Beitrag zur Entwicklung der "modernen Welt." RICHARD H. TILLY, Zur Entwicklung des Kapitalmarktes im 19. Jahrhundert. ERNST-WOLFGANG BÖCKENFÖRDE, Zum Verhältnis von Kirche und Moderner Welt. Aufriss eines Problems. JACOB KATZ, Die Anfänge der Judenemanzipation. WOLFRAM FISCHER, Rekrutierung und Ausbildung von Personal für den modernen Staat: Beamte, Offiziere und Techniker in England, Frankreich und Preussen in der frühen Neuzeit. ROLF GRAWERT, Historische Entwicklungslinien des neuzeitlichen Gesetzesrechts. RUDOLF BRAUN, Steuern und Staatsfinanzierung als Modernisierungsfaktoren. Ein deutschenglischer Vergleich. REINHART KOSELLECK, 'Neuzeit.' Zur Semantik moderner Bewegungsbegriffe. MANFRED RIEDEL, Historischer, metaphysischer und transzendentaler Zeitbegriff. Zum Verhältnis von Geschichte und Chronologie im 18. Jahrhundert. WOLF LEFENIES, Das Ende der Naturgeschichte und der Beginn der Moderne. Verzeitlichung und Enthistorisierung in der Wissenschaftsgeschichte des 18. und 19. Jahrhunderts. ECKART PANKOKE, Fortschritt und Komplexität. Die Anfänge moderner Sozialwissenschaft in Deutschland. HANS ULRICH GUMBRECHT, Zum Wandel des Modernitäts-Begriffes in Literatur und Kunst.

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Recent Deaths

JOHN A. CARPENTER, professor of American history and head of the department at Fordham University, died of cancer on May 15 at the age of 56 at White Plains Hospital.

From the moment he started teaching in 1948 at Mount Vernon Junior College in Washington, D.C., John Carpenter, a 1942 graduate of Harvard who took his doctorate at Columbia in 1954, established his credentials as a most gifted teacher and scholar. In 1957, he moved to Washington, Pennsylvania, to accept an associate professorship at Washington and Jefferson College. There he not only pursued his professional and scholarly concerns but also became an active member of the American Association of University Professors and the Pennsylvania Historical Association. In 1965 he came to Fordham, a mature scholar who quickly earned the friendship and respect of his colleagues and students.

Professor Carpenter's greatest contribution was undoubtedly in his field of specialization, the Civil War and Reconstruction. Students of the Freedmen's Bureau owe him an inestimable debt for his 1964 biography of Oliver O. Howard, entitled *Sword and Olive Branch*, and for his extended study of the Bureau agents, a project he managed to complete just before his death. In addition, he was vitally interested in military history, as his biography of U. S. Grant shows so well.

The recipient of grants from the National Endowment of the Humanities and the American Philosophical Society, Professor Carpenter was an able speaker who addressed audiences in many parts of the country. Because of his expertise on Lincoln and Grant, he was one of twenty-two scholars who appeared on the 1974 CBS television series, "The American Presidency: The Man and the Office."

John Carpenter's untimely death deprives the profession of an uncommonly kind and unassuming scholar. Not even his final bout with his dread disease kept him from finishing his manuscript on the agents of the Freedmen's Bureau, an accom-

plishment which was truly remarkable. After sifting through the records of literally hundreds of agents, high and low, he succeeded in writing a full account of their contributions—their strengths, their weaknesses, and their achievements. He is the first historian to have attempted anything like this work. It is bound to become standard and will stand as a monument to his great courage in the face of adversity.

HANS L. TREFOUSSE
*Brooklyn College,
City University of New York*

The death of HAROLD LEES FOWLER, former dean of the faculty of arts and sciences and professor emeritus at the College of William and Mary, on March 11, 1977, has left our historical company poorer and has removed a singularly gifted educator. Born in Boston, Massachusetts, on September 11, 1907, he received his bachelor's degree from Dartmouth College; in 1930 he obtained his master's degree at Harvard University, where he received his doctorate four years later. His dissertation, a lucid, balanced study of Sir Edward Seymour, Speaker of the House of Commons and Tory politician, marked the beginning of a lifelong preoccupation with Stuart history. Appointed assistant professor at William and Mary at the behest of Richard L. Morton, departmental chairman, he spent the entirety of his academic career in the college at Williamsburg, except for three years service in the armed forces during World War II and, later on, a year leave of absence to teach at the California Institute of Technology. Although by his training and native endowment he was admirably qualified to engage in research, his output of historical publications was, for the most part, limited to occasional reviews together with an article in the *Dictionary of American Biography, Supplement Three*. An intended biographical history of Charles II's Cavalier Parliament (1661–1678), the materials for which he gathered in the Huntington Library, had to be abandoned because of administrative respon-

sibilities which he assumed soon after returning to Virginia.

As a member of the college faculty, Fowler made a lasting contribution to the school's reputation by his years of service in the classroom. He inaugurated an introductory survey course in European history, modeled after History I at Harvard, which became a centerpiece of the curriculum for thousands of students, thanks to his skill in lecturing and his talent for luminous exposition of the past. And when he stood behind the lectern he seemed ever mindful of Sir Henry Irving's wise admonition: "There is a king in every audience. *Play to him!*" He was, in truth, a born teacher, a dedicated man with an orderly and precise mind, who gave freely of himself in counsel with undergraduates or colleagues, while carrying an instructional load that would stagger the imagination of today's academic generation. He played a prominent role in faculty affairs, particularly during the 1950s and 1960s. He served a three-year term as secretary, chaired a committee which investigated a reverberating scandal in the athletic program, and steered through the faculty by overwhelming vote the committee's resolution (based upon its findings) that the college should henceforth de-emphasize professionalism in athletics. A staunch advocate of academic freedom, he fought tooth and nail to prevent the violation of student and faculty rights by an authoritarian president—this notwithstanding the risk of possible dismissal.

Beginning in 1959 he became increasingly involved in administrative matters, first as department head and then, five years later, as academic dean, a post which he held for a decade till his retirement from the college. He conducted the deanship with quiet efficiency; he enhanced its stature by his leadership of the faculty, his concern for quality education, and his unremitting devotion to duty. Amidst the day-to-day administrative grind he retained his zest for historical study and, more important, he continued to teach his course in Tudor and Stuart England, one of the best and most popular offerings in the departmental curriculum.

BRUCE T. MCCULLY
Professor Emeritus
College of William and Mary

ALFRED J. HENDERSON, professor of history at MacMurray College, Jacksonville, Illinois, died in Jacksonville on November 4, 1977 after a long illness. He retired from teaching in 1971 after twenty-seven years as professor of history and head of the department of history and government.

He was born in England, March 10, 1905, came to the United States with his parents and brothers

in 1910, and became a naturalized citizen in 1917. He and his family lived in Rochester, New York, where he attended local schools and earned his A.B. and A.M. degrees at Rochester University as a student of Dexter Perkins. He went to Duke University for his Ph.D. in English history where his major professor was William T. La Prade. At Duke he was elected to Phi Beta Kappa. Henderson was a specialist in the Age of Walpole, and his dissertation, *London and the National Government, 1721-1742*, was published by Duke University Press in 1946, and a reprint was issued by Porcupine Press in 1975. He was elected a Fellow of the Royal Historical Society in 1957.

Al Henderson (no one ever called him "Alfred") was dedicated to the small church-related college as an educational instrument. He taught at Berea College and Kentucky Wesleyan College before coming to MacMurray in 1944, although he enjoyed an occasional summer of teaching at Rochester and Duke. At MacMurray he was active in faculty and college administration, serving on the curriculum committee for many years. He was the chairman of the committee on scholarships and fellowships. In the latter capacity he influenced and helped many young people to pursue graduate study. Under his leadership, the history and government department developed a strong program of senior theses and departmental written examinations. Under his inspiration, the department staff taught classical historical method, so that MacMurray students had a head start when they attended graduate school. As a teacher, Al was an effective lecturer, and never went to class without meticulous preparation.

Al was active in community and church in Jacksonville. He was a long-time member of Kiwanis Club and served a term as president. He supported the YMCA and the Field Service Committee in their student exchange programs. For many years he was representative of MacMurray College with the United Fund. In Grace Methodist Church, he was a leading layman and also did occasional preaching. He also devoted much time to local historical affairs. As president of the Morgan County Historical Society, he applied the teachings of his craft to the uplift of county and city history, bringing leading men as speakers, and encouraging amateurs to prepare and deliver papers.

He contributed articles on local history to the *Illinois State Historical Society Journal* and to the *Jacksonville Journal-Courier*. He was a member of the editorial board of the book *Morgan County in the Twentieth Century*. He was also a contributing member of the Jacksonville Sesquicentennial-United States Bicentennial Committee.

Henderson was a member of The Club, an an-

cient men's literary society in Jacksonville, where he delighted his fellow members with his brilliantly prepared papers. In his later years, he and his wife traveled extensively in Europe and Asia, and his skill with a camera provided his friends at home with vivid and beautiful slide shows, which were always illuminated with his remarks on the history of whatever part of the world he was illustrating.

In 1939, Alfred J. Henderson was married to Elizabeth Aldridge of Durham, North Carolina. She was the daughter of a mathematics professor at Duke University and a long-time executive secretary of the Duke Alumni Association. She is an accomplished soprano singer, and Al had a resonant baritone voice. They both sang in the Duke choir. Music was always a part of their lives. Their two children, James and Alice, have followed their parents into academic and professional careers.

Al Henderson, like his father before him, was an enthusiastic gardener, and the flower beds and lawns of his home were one of the show places of Jacksonville. Elizabeth and Al delighted in entertaining students, faculty, bridge club members, and townspeople in their home.

WALTER B. HENDRICKSON
Professor Emeritus
MacMurray College
Jacksonville, Illinois

GILBERT M. HILL, professor of history at Clarion State College, Clarion, Pennsylvania, died on February 4, 1978, at the age of 59. He had come to Clarion in 1970 as chairman of the newly created Department of History which had just been severed from a combined Department of Social Science. A history club and a chapter of Phi Alpha Theta were established under his direction, and he worked tirelessly to ensure their success, as well as to develop the master's program begun shortly before he arrived. He served the college community as a member of the Faculty Senate, the Honors Colloquium, and the Liberal Arts Commission.

Before coming to Clarion Dr. Hill had held various administrative and teaching posts at Eastern New Mexico University in Portales, Towson State University in Towson, Maryland, and Temple University in Philadelphia.

Dr. Hill earned his undergraduate degree at Temple, his master's at the University of Pennsylvania, and his doctorate at Indiana University, Bloomington. He was a member of several learned societies and his professional interests embraced many aspects of American and European history. He was respected and admired by students in both the undergraduate and graduate programs, and by

his colleagues in the department and the college as a whole. His course in American labor history was popular with students in the business program at Clarion. He was a man of gentle kindness and patient consideration, supportive of the needs and interests of his colleagues and students and ever willing to encourage their projects and plans. His absence will long be acutely felt at the college and by the family that survives him; his widow Bernice, his daughter Claire Ellen, and his sons Richard Meredith and Charles Thomas.

ZOE SWECKER
Clarion State College

J. WESLEY HOFFMANN, former head of the history department at the University of Tennessee died February 21, 1978 at age 82.

Professor Hoffmann came to Tennessee in 1937 and was department head from 1942 until his retirement in 1965. He had taught previously at Chicago, Montana State, Iowa, and Morningside.

He received his B.A. from Minnesota and his M.A. and Ph.D. from the University of Chicago where he was a student of Bernadotte Schmitt (who was a Knoxville with a Tennessee degree). Hoffmann also was instrumental in establishing a history scholarship to honor his former mentor's memory. He had studied in Berlin and Bonn on German government fellowships before the advent of Hitler and had spent considerable time in Europe on various assignments after World War II. He was a navy veteran of World War I and was one of the civilians responsible for the operation of the Army Air Corps program at Tennessee during World War II. An able and respected organizer, Hoffmann nearly tripled the size of his department. He was always concerned with improving the university's academic image and was equally active in civic affairs, serving as president of several organizations. He was a specialist in Central European history and was one of the founders and the first chairman of the European History Section of the Southern Historical Association.

ARTHUR G. HAAS
The University of Tennessee,
Knoxville

LILLIAN ADELE KIBLER, Professor Emeritus of History at Converse College, died on May 11, 1978, in Newberry, South Carolina, where she was born August 16, 1894. After receiving degrees from Winthrop College and Randolph-Macon Woman's College, she taught for many years in South Carolina high schools, and earned a reputation for teaching excellence. In 1937 she received her master's degree in history from Columbia University and began doctoral study under the direction of

Allan Nevins, who later spoke of her as a woman of "originality of mind, amounting to unusual intellectual distinction." She completed her doctoral degree in 1943 and later published her dissertation on *Benjamin F. Perry: South Carolina Unionist* (1947).

In 1942, Lillian Kibler joined the Converse College department of history, where she soon became department chairperson, remaining in that position until her retirement in 1962. In all that time she was recognized by faculty and students alike for her excellent teaching and scholarly interests and integrity. In addition to her publication on Perry, Professor Kibler was known for her work on William H. Trescott and for her *History of Converse College, 1889-1971* (1973). A master teacher and excellent friend and colleague, Lillian Kibler will be missed by her many students, friends, and former colleagues.

FREDERICK F. RITSCH
Converse College

MARGARET STERNE, professor of history at Wayne State University, died in Detroit on November 6, 1977. She is survived by her husband, Bernhard, two sons, and five grandchildren.

Born in Frankfurt, Germany, on June 18, 1904, Sterne completed her undergraduate and graduate work at the University of Berlin, Wayne State

University, and the University of Michigan. Before coming to Wayne State University she was corresponding editor of the Ullstein publishing house in New York and Detroit. She was rotogravure editor for *The Detroit News* from 1925 to 1934 and served as publicity director for the U.S. Treasury Department, Michigan War Finance Committee, from 1942 to 1944.

In 1952 Professor Sterne was awarded a Fulbright scholarship for research in Vienna and lectured in Germany and Austria. In 1959 she received the Order of Merit First Class from the Federal Republic of Germany. She published several articles in historical journals and recently completed a biography of William R. Valentiner, Director of the Detroit Institute of Arts from 1924 to 1945.

In 1971, after 29 years of teaching, Margaret Sterne retired from Wayne State University but continued her professional work as a research associate at the Detroit Institute of Arts.

Margaret Sterne will be remembered by her students as a superb teacher, a kindly and compassionate counsellor, and a professor who was sincerely interested in them. Her colleagues know what she did for the department, the university, and the city of Detroit. It was a great deal indeed.

GOLDWIN SMITH
Wayne State University

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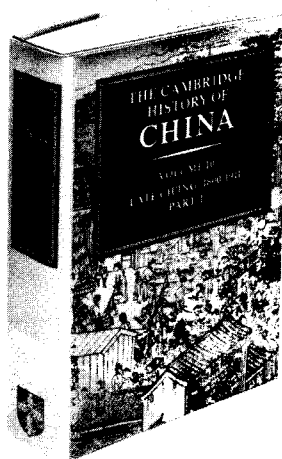
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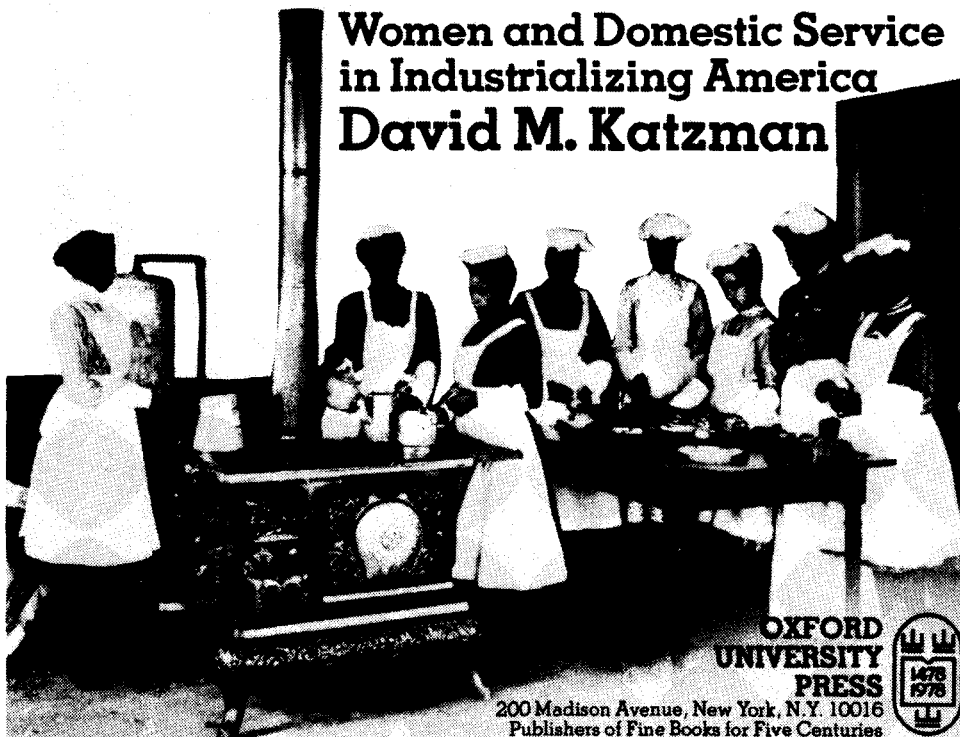
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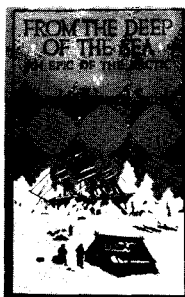
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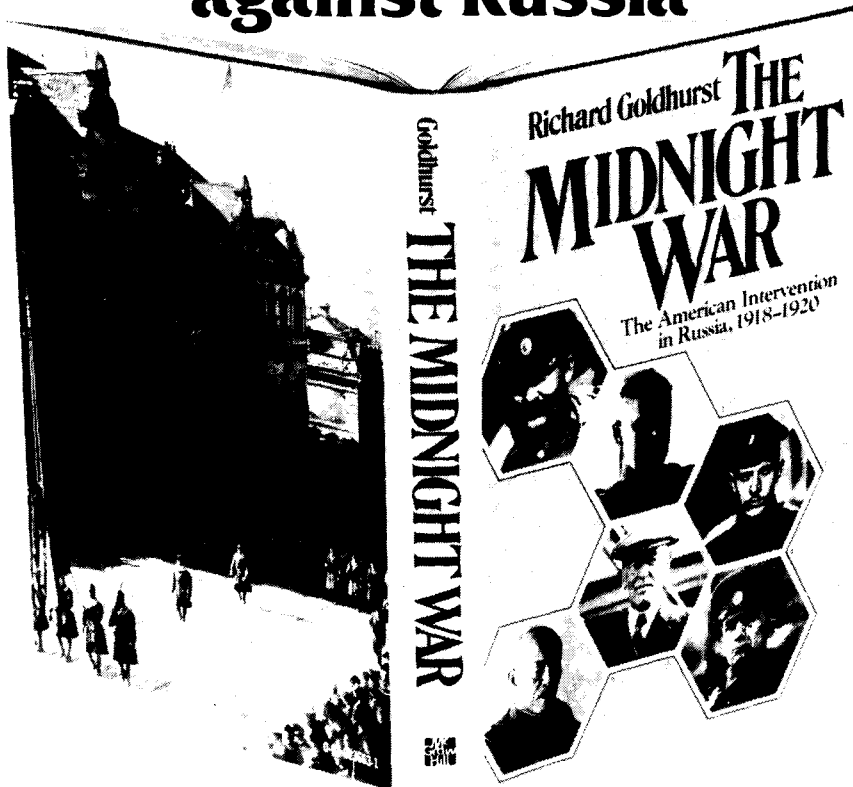
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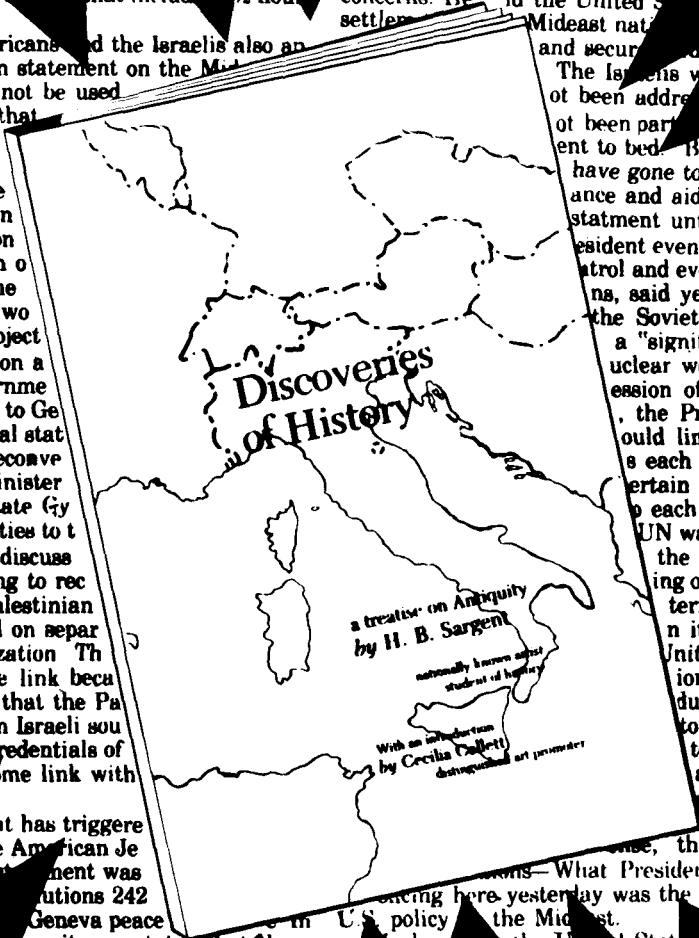
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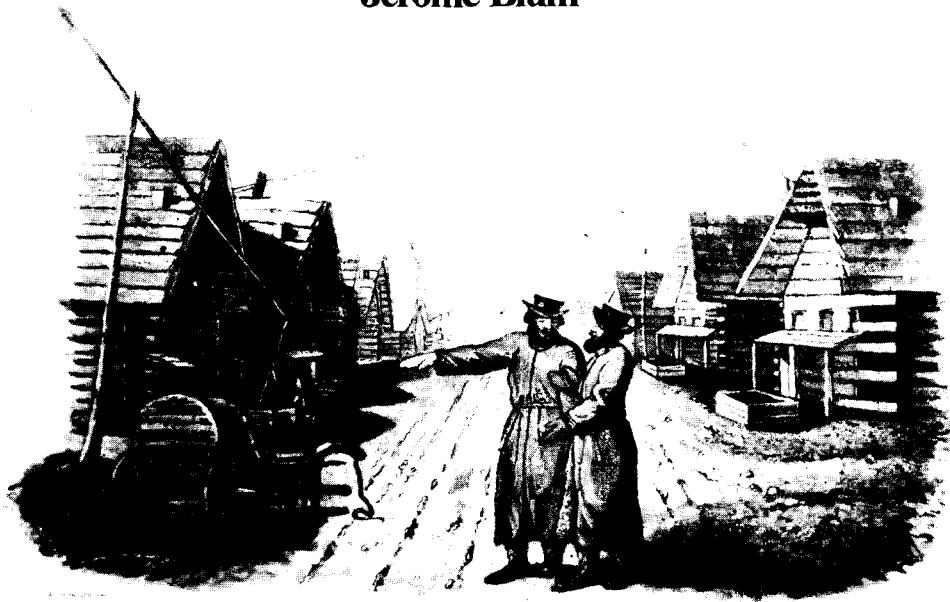
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